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Sources of Retroactive Inhibition in Pigeon Short-Term Memory

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A delayed matching-to-sample (DMTS) task was used to test several hypotheses about sources of retroactive inhibition (RI) in pigeon short-term memory. After being trained on DMTS with colors and patterns, pigeons were tested for RI in Experiments 1 and 2 by presenting interpolated stimuli designed to test the influence of the dimensions of similarity, saliency, familiarity, complexity, and degree of illumination. Of these dimensions, the only one which proved to be a significant source of RI was degree of illumination. Experiments 3 and 4 demonstrated a direct relationship between degree of illumination emitted from either the center key or houselight and the amount of RI. Possible mechanisms which could explain the strong influence of interpolated illumination upon retention in animals are discussed.

With the recent development of an in- erest in processes of memory in animals, the lelayed matching-to-sample (DMTS) task ias become popular as a technique for the tudy of short-term memory (STM) in nonkeys (D'Amato, 1973; Moise, 1970) nd in pigeons (Roberts, 1972; Zentall, 973). In general, it has been found that ccuracy of matching declines as the delay nterval is lengthened, and this has been ttributed to loss of memory for the sample timulus. A question of major theoretical nterest concerns what factors affect the rate f short-term forgetting. On the basis of tudies of human memory, we might expect hat forgetting would be increased by in- roducing extra information into the memory ystem either prior to the presentation of he sample stimulus (proactive inhibition) or following the presentation of the sample stimulus (retroactive inhibition; RI).

Several studies carried out recently using proaction paradigms have revealed evidence of interference with delayed matching. Using an intratrial paradigm, Grant and Roberts (1973) presented an interfering stimulus prior to presentation of a sample stimulus and then presented both stimuli for choice at the end of a delay. The interfering stimulus lowered overall performance relative to a control condition but did not affect the rate at which the sample was forgotten across delays. Grant (1975) used an intertrial paradigm to study proactive inhibition by presenting conflicting information on two or more successive trials. When the stimulus used as the sample on interference trials became the incorrect choice on a final test trial, amount of forgetting on the test trial increased relative to a control condition over a 6-sec delay period. These interference effects have been accounted for by a theory of competition between memory traces of the

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correct and interfering stimuli (Roberts & Grant, in press).

In the research reported here, we turned our attention to the problem of RI in pigeon STM. Available information suggests that stimulation appearing during the delay interval interferes with retention in both monkeys and pigeons. D'Amato (1973) found that ambient illumination severely interferes with retention in capuchin monkeys relative to a darkness condition but that the presentation of either white noise or monkey vocalizations produces no forgetting. Requiring monkeys to perform a reaction time task has been found to produce RI which increased as the number of responses required increased (Moise, 1970). Other studies have presented stimulation during the delay on the display panel or key on which the sample had been shown initially. Jarvik, Goldfarb, and Carley (1969) trained monkeys on DMTS using red and green stimuli and then studied RI by presenting during the delay either neutral, negative, or positive stimuli. The neutral stimulus was a white light, the negative stimulus was the incorrect member of the test pair, and the positive stimulus was the correct member of the test pair (i.e., the sample stimulus). Comparisons of these conditions with a blank-interval control condition showed little effect of the neutral stimulus, a slight facilitation produced by the positive stimulus, and a marked reduction in performance resulting from presentation of the negative stimulus. As Zentall (1973) pointed out, changes in performance produced by presentation of the correct or incorrect test stimuli during the delay are difficult to interpret as retention effects. Rather, they may arise from a failure of the animal to understand which stimulus is to be matched. A tendency to respond to the most recently appearing stimulus would produce the effects of positive and negative stimuli reported by Jarvik et al. The failure of Jarvik et al. to find any effect of the neutral white stimulus suggests that stimuli which are dissimilar to the test stimuli may have no effect on retention. Zentall (1973) pursued this problem with pigeons. After training birds on delayed matching with re and green stimuli, he ran test sessions i which either a vertical white line on a blac background, a yellow field, or darkness appeared on the center key during a 5-se delay. Both the line and yellow field pro duced poorer retention than the dark ke control, and performance with the yello field was significantly inferior to that wit the line. Zentall suggested that the differ ence between the line and yellow field coul be interpreted as an effect of either simi larity or saliency. That is, the greate similarity of the yellow color to the sampl stimuli (colors) may have led to its greate influence, or yellow may simply have bee a more distracting stimulus than a whit line, independent of its similarity to th sample.

EXPERIMENT 1

In this experiment we attempted to answe two questions raised by Zentall's (1973 experiment concerning the properties o interpolated stimuli that produce RI. T first question is that of similarity vers saliency. If we divide stimuli into tw classes, colors and black-and-white pattern a similarity principle argues that inte polated stimuli from either class would i terfere more with delayed matching stimuli from its own class than with tho from the other class. The saliency princip would argue that the more distracting cla of stimuli, presumably colors, would sho greatest interference with delay matching i both classes of stimuli. We trained bir on DMTS using both classes of stimuli an then tested for RI by presenting trials o which the interpolated stimuli were fro the same or different class as the matchi stimuli. Greater RI when the matching an interpolated stimuli are either both colo or both patterns would support a similari principle, and greater overall RI wi colored interpolated stimuli than with pa terned interpolated stimuli would support dimensional saliency principle.

The second question concerns the novelt of the interpolated stimuli. Zentall's (1973 yellow field and white line were stimul hich his pigeons had never encountered efore in the experimental situation. An nteresting question arose as to whether igeons would show more or less RI if the nterpolated stimuli were familiar to the irds as stimuli used in previous DMTS raining. From an information-processing oint of view, it might be argued that miliar stimuli, having been encoded for etention previously, would be more likely o be encodel into the memory system than ovel stimuli, and that the presence of a econd trace in memory would interfere ith maintenance of the trace of the sample STM. On the other hand, one might old the position that novel stimuli produce ore RI than familiar stimuli because their ewness may cause them to be attended strongly and thus interfere with maintenance of memory for the sample stimulus. o deal with this issue, we gave birds initial MTS training with three colors and three lack-and-white patterns. During RI test rials, one of the three colors and one of the hree patterns were presented as familiar nterpolated stimuli, and a previously unused olor and pattern were presented as novel nterpolated stimuli.

ethod

Subjects. Ten adult Silver King pigeons were sed. The birds were maintained at 80% of their ee-feeding weight throughout the experiment.

Apparatus. Two operant chambers for testing igeons were used, measuring 31 cm × 35.5 cm floor dimensions) × 35.5 cm (wall height). Each hamber contained a panel with a row of three ecking keys (spaced 8 cm apart, center to center, the height of a pigeon's head) and a centrally cated grain aperture below the row of keys. ultistimulus projectors mounted behind each ey illuminated the keys with colors or with black-and-white patterns. All of the events of a day's ssion of trials, stimulus presentations, delays, nd intertrial intervals, were controlled by relays, mers, and counters, which were activated by a aper-tape reader. Printing counters recorded oices on each trial.

Four colored fields and four black-and-white atterns were used as stimuli. The colored fields ere red (635 nm), yellow (590 nm), green (570 m), and blue (500 nm). Two of the patterns ere three white, vertical stripes against a black ackground and three white horizontal stripes gainst a black background. The stripes were each 3 mm wide and spaced 3 mm apart, with the central stripe running the diameter of the key (26 mm) and the two outside stripes measuring 23 mm in length. The total area taken up by the stripes was 216 mm^2. The other two patterns were made up of white dots against a black background, with each dot measuring 5 mm in diameter. One pattern consisted of three dots placed in a diagonal line, spaced 2 mm apart; the other pattern consisted of five dots spaced 2 mm apart placed in a cross pattern, three vertical dots and three horizontal dots with a common central dot. The diagonal dots pattern occupied 58.9 mm^2 of the key area, and the crossed dots occupied 98.2 mm^2. The illumination emitted by stimuli was measured with a Tektronix J16 Digital Photometer at a distance of 15 cm from the key. The readings for the colored fields, red, yellow, green, and blue, were 16, 58, 15, and 11 cd/m^2, respectively. The luminance of the fully illuminated white key was 75 cd/m^2. The striped patterns (horizontal and vertical) illuminated 40.7% of the white key surface area. Diagonal dots and crossed dots patterns illuminated 11.1% and 18.5% of the white key, respectively.

Procedure. Following magazine training, each bird was shaped to peck a white center key and then was trained to peck a white center key in order to turn on white side keys, either of which then delivered 3 sec of grain reinforcement when pecked. Once a bird was pecking white keys satisfactorily, trials were given in which either colors (red, green, or blue) or patterns (vertical stripes, horizontal stripes, or diagonal dots) were presented with the same color or pattern appearing first on the center key and then on the side keys when the center key was pecked; as with the white stimuli, a peck on either side key yielded reinforcement.

Training on simultaneous matching-to-sample (SMTS) began when a bird was readily pecking each of the colors and each of the patterns. An SMTS trial was initiated by the onset of a white center key. A single peck on the white key introduced the sample stimulus on the center key, either a color or pattern. A fixed number of pecks on the sample stimulus turned on the side keys, one containing a match-to-the-sample stimulus and the other containing a nonmatching stimulus, a color if the sample was a color and a pattern if the sample was a pattern. If the matching side key was pecked, reinforcement was delivered followed by a 10-sec intertrial interval in darkness. If the nonmatching side key was pecked, a correction procedure went into effect in which there was a 6-sec time-out period in darkness followed by relumination of the center and side keys for another choice. Correction continued until the matching side key was pecked.

There were 48 trials of SMTS training given each day, with any given day's trials consisting solely of color trials or of pattern trials. The animals were tested alternately on colors and patterns, color testing on odd days and pattern testing on even days. On color trials, one of two combinations of stimuli was presented on the keys, red and blue or red and green, with red being the sample and reinforced matching stimulus on half the trials and blue or green being the sample stimulus on the other half of the trials. The four types of trials thus generated were presented in 12 4-trial blocks, the orders of trial types randomized within blocks under the overall restrictions that each color appear equally often as the matching and nonmatching stimulus on the right and left side keys, and that no side key contain the matching stimulus for more than three trials in succession. The same procedures were used to generate 48-trial sequences of pattern stimuli. For pattern trials, the stimulus pairs were always either horizontal stripes and vertical stripes or horizontal stripes and diagonal dots. For both color and pattern stimuli, there were four different sequences of 48 trials, and each bird was tested repeatedly on all four sequences, each time in a different order.

On the initial day of SMTS training with each class of stimuli, a fixed ratio of one peck was required on the sample stimulus in order to produce the side keys. Thereafter, the fixed ratio was increased by two each day within each class of stimuli until a fixed ratio of 10 was reached. Subjects achieved criterion when they made 90% or better correct responses over a 5-day period for a given class of stimuli. Since each of the birds reached criterion sooner on colors than on patterns, each bird continued training on patterns after completing training on colors. When an animal had reached criterion with the patterned stimuli, it was given SMTS training for 8 more days with color and pattern stimuli used on alternate days. Two modifications of procedure were introduced on these days: The reinforcement time was lowered from 3 sec to 2 sec, and noncorrection replaced correction so that an incorrect response now produced the intertrial interval.

Following completion of SMTS training, a bird was placed on 0-sec DMTS training for 8 days, 4 days with colors alternating with 4 days with patterns. The procedures were the same as in SMTS, except that (a) presentation of the sample stimulus was changed from ratio control to timer control (Roberts & Grant, 1974), the sample stimulus appearing for 6 sec on each trial regardless of the number of pecks that were made to it; and (b) the sample stimulus went off at the instant the side keys were illuminated. After completion of 0-sec DMTS, each pigeon was given 12 color days alternating with 12 pattern days of training with delays of 0, 2, 5, and 10 sec between sample termination and onset of the side keys. Each delay was tested 12 times within a day's session of 48 trials and was paired equally often with each type of trial. In the final stage of preliminary training, all birds were given DMTS training with a 5-sec delay on all trials. This training was given for 7 color (C) days and 11 pattern (P) days occurring in the following order: CPCPCPCPPCPPCPPCPP. This procedure was used to bring performance with patterned stimuli close to that with colored stimuli and still maintain color performance at a high level.

Experimental testing for RI was carried out on alternating color and pattern days. On color days, only the colors red and green were used as sample and testing stimuli, each color being the sample on half the trials. The same procedure was used on pattern days, with horizontal and vertical stripes used as stimuli for the matching task. There were 50 DMTS trials given each day, and the delay was 5 sec on each trial. Ten of these trials were control trials on which the center key was dark during the delay. The other 40 trials were RI test trials on which one of four stimuli appeared on the center key for the 5-sec delay. These stimuli were a yellow field, a blue field, a pattern of diagonal dots, and a pattern of dots forming a cross. Each of the interpolated stimuli was tested for 10 trials on both color and pattern days. Six sequences each of 50 color trials and of 50 pattern trials were constructed in which (a) every interpolated stimulus occurred equally often with each sample stimulus, (b) position of the correct side key was balanced across types of trials, and (c) the different types of trials appeared in a random order subject to the restrictions that no sample stimulus was correct more than 3 trials in a row and that no side key could be correct more than 3 trials in a row. The experiment was carried out for 12 days. Each of the birds was tested once with each sequence of color and pattern trials, the sequences being tested in a different order for each animal. For reasons that will become clear in the presentation of the data., it was necessary to give the birds 6 more days of testing with the patterned matching stimuli using a 2-sec delay. Thus, following the 12th day of testing on the originally planned portion of the experiment, all birds were given 5 days of regular DMTS training with a 2-sec delay and then were tested for 6 days with interpolated stimuli appearing during the 2-sec delay.

The interpolated stimuli used in RI testing were chosen to provide answers to the questions posed in the introduction. Thus, when the sample was a color, both stimuli from the same class (blue and yellow) and from the different class (diagonal and crossed dots) were interpolated stimuli, and these interpolated stimuli served just the opposite roles when the sample was a pattern. Also, in testing with both colored and patterned sample stimuli, two of the interpolated stimuli were familiar (blue field and diagonal dots), and two of the interpolated stimuli were novel (yellow field and crossed dots).

Results

Preliminary training. In acquisition of the SMTS task, all birds reached criterion sooner with colors than with patterns; for colors, a range of 8 to 13 days was required with a mean of 10.5 days, while for patterns, the range was 19 to 41 days with a mean of 31.4 days. These results seem to be in general agreement with those of Farthing and Opuda (1974), who found that pigeons required about four times as many sessions to learn matching-to-sample with black-and-white forms as with colors. Over the subsequent 8 days of SMTS training with reinforcement time lowered to 2 sec and correction eliminated, the subjects made an average of 91.9% correct responses on the 4 color days and 87.3% correct responses on the 4 pattern days: the difference between these percentages was not significant, $t(9) = 1.50$, $p > .05$.

When the birds were transferred to 0-sec DMTS, performance with colors (97.7% correct responses) was significantly better than that with patterns (85.7% correct responses) over the 4 days of testing with each class of stimuli, $t(9) = 3.11$, $p < .05$. The multiple-delay testing given next consisted of 12 days of testing with each class of stimuli, using delays of 0, 2, 5, and 10 sec. Performance improved over the 12-day period for both classes of stimuli, more for colors than for patterns; matching accuracy increased from 65.5% correct on the first 4 days to 79.8% correct on the last 4 days for colors and increased from 62.2% correct to 66.9% correct for patterns. With both classes of stimuli, negatively accelerated forgetting curves, typical of pigeon DMTS, were found when accuracy was plotted against delay. As in 0-sec DMTS, overall performance was significantly higher with colors, 72.8% correct, than with patterns, 64.8% correct, $t(9) = 5.70$, $p < .01$. In the final stage of preliminary training, all subjects were given DMTS training with a 5-sec delay on all trials, with 7 days devoted to color stimuli and 11 days to pattern stimuli. The final levels of performance on Days 7 and 11 for color and pattern stimuli were 79.0% and 75.4% correct responses, respectively. These percentages did not differ statistically, $t(9) = .92$, $p > .05$.

RI testing. Percentage of correct responses on control trials and on trials with interpolated stimuli are presented in Table 1. For each class of sample stimuli, percentages are presented separately for each of the two samples used, along with the mean for the class as a whole. These percentages represent performance over 6 days of testing, with each percentage for individual sample

TABLE 1

Percentage of Correct Responses for Color and Pattern Interpolated Stimuli as a Function of Color and Pattern Sample Stimuli

		Interpolated stimuli			
Sample stimuli	Control	Familiar color (Blue)	Novel color (Yellow)	Familiar pattern (Diagonal dots)	Novel pattern (Crossed dots)
Color (delay = 5 sec)					
Green	85.7	91.0	59.3	72.7	66.7
Red	77.0	38.7	72.3	73.0	68.3
M	81.4	64.8	65.8	72.8	67.5
Pattern (delay = 5 sec)					
Vertical stripes	65.7	57.0	53.3	63.0	59.0
Horizontal stripes	64.7	48.3	47.7	52.3	47.3
M	65.2	52.6	50.5	57.6	53.2
Pattern (delay = 2 sec)					
Vertical stripes	73.7	58.7	53.0	64.3	53.3
Horizontal stripes	74.7	58.0	61.0	61.0	64.7
M	74.2	58.4	57.0	62.6	59.0

stimuli based on 300 observations. The data for the originally planned portion of the experiment, with delay set at 5 sec, are presented in the top six rows of numbers. It can be seen by examining the means that strong RI effects are evident with both color and pattern sample stimuli with all four of the interpolated stimuli used. That is, with each of the interpolated stimuli accuracy of matching was inferior to that in the control condition. There seems to be little difference among three of the interpolated stimuli, the familiar color (blue), the novel color (yellow), and the novel pattern (crossed dots), but there was a somewhat better performance with the familiar pattern (diagonal dots).

An analysis of variance (ANOVA) was performed on mean percentages for both the color and pattern data, with condition of interpolation (interpolated stimuli and the control condition) being one factor and days of testing being another. The analysis showed that condition of interpolation was significant for both colors, $F(4, 36) = 9.93$, $p < .01$, and patterns, $F(4, 36) = 8.78$, $p < .01$, but that the days effect and the Condition of Interpolation × Days interaction were not significant with either class of sample stimuli ($p > .05$ in all cases). As a further test for specific RI effects, each interpolated stimulus was tested against the control by t test, and it was found that control percentages were significantly higher in every comparison for both color and pattern sample stimuli ($p < .05$ in all cases). Further t tests were made between pairs of interpolated stimuli to test for differences in degree of RI. With the color sample stimuli, the only significant differences which appeared were between the familiar pattern and the familiar color, $t(9) = 3.02$, $p < .02$, and between the familiar pattern and the novel color, $t(9) = 3.23$, $p < .02$; with the pattern sample stimuli, there were significant differences between the familiar pattern and the familiar color, $t(9) = 2.68$, $p < .05$, the familiar pattern and the novel color, $t(9) = 2.64$, $p < .05$, and the familiar pattern and the novel pattern, $t(9) = 2.93$, $p < .02$. Thus, the familiar pattern (diagonal dots) generally showed less RI than the other interpolated stimuli.

Performance with patterns as sample stimuli was clearly inferior to that with colors as sample stimuli, and with interpolated stimuli the percentages for patterned sample stimuli closely approach a chance level of performance (50%). It may be argued that a floor effect is masking differences between interpolated conditions which would otherwise appear. In order to raise performance with patterns, then, birds were given an additional 6 days of testing with the delay between sample offset and test only 2 sec. In the bottom three rows in Table 1, it can be seen that lessening delay to 2 sec did raise the performance level with pattern sample stimuli under all conditions of interpolation. However, the basic pattern of results observed with both color and pattern sample stimuli at a 5-sec delay remained unchanged. A Conditions of Interpolation × Days × Subjects ANOVA on the 2-sec delay data showed a significant effect of conditions of interpolation, $F(4, 36) = 7.09$, $p < .01$, but no significant effect of days or of the Condition of Interpolation × Days interaction. Again, performance with each of the interpolated stimuli was significantly inferior to that with the control. Finally, t tests among interpolated stimuli revealed only one significant difference, that between the familiar pattern and the novel color, $t(9) = 2.93$, $p < .02$.

An interaction between specific sample stimuli and type of interpolated stimulus can be seen within the class of color sample stimuli. When the interpolated stimulus was blue, matching was markedly higher with a green sample than with a red sample. Just the opposite effect occurred, however, if the interpolated stimulus was yellow. With patterns as interpolated stimuli, little difference between green and red samples appeared. To determine whether a particular interpolated stimulus significantly biased performance in favor of one sample stimulus, the difference between percentages of correct responses on the two samples for each interpolated stimulus was tested against the corresponding difference for the control condition. It was found that blue significantly biased performance in favor of the green sample, $t(9) = 8.85$, $p < .01$, and that yellow significantly biased performance in favor of the red sample, $t(9) = 2.61$, $p < .05$, but no significant effects of patterned interpolated stimuli were found. The same analysis was made for all four interpolated stimuli with patterns as sample stimuli, and no significant biasing effects were found with either the 5-sec or 2-sec delay data ($p > .05$ in all cases).

Pecking on interpolated stimuli. The number of pecks which birds made upon the center key during the presentation of interpolated stimuli was recorded during this experiment. When the number of pecks per trial was averaged over the colored and patterned sample stimuli at the 5-sec delay, the mean pecks were .33, 5.94, 5.18, 5.12, and 6.18 for the control, yellow field, blue field, diagonal dots pattern, and crossed dots pattern, respectively. Clearly, the illuminated center key was pecked more frequently than the darkened control key, and there was a tendency to peck both the yellow field and the crossed dots pattern more often than either the blue field or the diagonal dots pattern. The possibility that degree of responsivity to the interpolated stimulus and degree of RI are related was not supported in Zentall's (1973) study, in which a nonsignificant correlation was found between these variables. A similar analysis on these data between overall pecking rate to interpolated stimuli and RI scores (control–experimental correct responses) also yielded a nonsignificant correlation, $\rho\ (9) = -.36$, $p > .05$.

Discussion

The results reported in this experiment are in agreement with those reported by Zentall (1973). In both experiments, colored and patterned interpolated stimuli significantly inhibited the pigeon's short-term retention of memory for color stimuli. Specifically, Zentall found that a yellow field and a vertical white line produced RI and that the yellow field produced more RI than the line. If the diagonal row of dots used in this experiment is considered equivalent to a line, then exactly the same results were produced in this experiment. That is, both the yellow field and the diagonal dots produced significant RI of delayed matching with colored sample stimuli, and the performance with the yellow field was significantly inferior to that with the diagonal dots. An examination of the total findings of this experiment suggests, however, that neither of the explanations offered by Zentall to account for the differential influence of these two interpolated stimuli is accurate. The similarity principle suggests that each class of interpolated stimuli should interfere more with retention of sample stimuli from its own class than with those from a different class. When colored sample stimuli were used, blue and yellow interpolated stimuli produced more RI than the diagonal dots pattern but did not differ from the crossed dots pattern. When patterned sample stimuli were used, patterned interpolated stimuli did not produce more RI than colored interpolated stimuli; to the contrary, the diagonal dots pattern produced less RI than both of the colored stimuli. Thus, there is clearly no tendency for either color or pattern as classes of interpolated stimuli to interfere more with retention of stimuli from their own class than with those from the other class. A dimensional saliency principle argues that colors have more distracting power than patterns and thus should produce more RI than patterns, given any set of sample stimuli. The data do not support this principle, since performance with crossed dots (a pattern) as an interpolated stimulus never differs significantly from performance with blue or yellow as interpolated stimuli.

The familiarity–novelty variable was introduced into this experiment to determine whether a stimulus previously used in the matching task would produce more or less RI than a stimulus not previously encountered in the experimental situation. The finding that diagonal dots generally produced less RI than crossed dots suggests the possibility that novelty might be an important source of RI. However, the failure to find a difference between the blue familiar color) and yellow (novel color) interpolated stimuli leads to a rejection of this possibility. We seem to be left with the conclusion that there is some property of the diagonal dots pattern that makes it a less effective inhibitor of retention than the blue and yellow fields and the pattern of crossed dots. This property does not seem to be similarity to the sample, dimensional saliency, or familiarity. Some alternative possibilities may be offered: (a) One dimension which differentiates diagonal dots and crossed dots is figure complexity, since a cross has more turns or angles than a straight line. Perhaps the more simple a pattern is, the less RI it produces. Unfortunately, this dimension would not relate the patterns to the colored fields. (b) A dimension which could encompass all of the interpolated stimuli used is the sheer amount of surface area on the center key which emits light. Since the diagonal dots pattern occupied less of the key area than both the colored fields and the crossed dots pattern, this factor could explain the difference in RI. A finding which could be interpreted as supportive of this suggestion is that monkeys have been found to show progressively worse performance on a spatial delayed-response task as the degree of chamber illumination during the delay was increased (Malmo, 1942).

Although similarity between classes of stimuli as a whole was not a significant factor producing RI, analysis of the interaction between specific sample stimuli and interpolated stimuli suggests that similarity within the color dimension was an important factor. Very strong RI effects were found when the sample was green and the interpolated stimulus was yellow or when the sample was red and the interpolated stimulus was blue. Conversely, little RI—in fact, some retroactive facilitation—was found when a green sample was followed by a blue interpolated stimulus or a red sample was followed by a yellow interpolated stimulus. These findings are akin to those of Jarvik, et al. (1969) with monkeys and can be understood readily in terms of stimulus generalization and a tendency for the pigeon to base his response on the stimulus most recently seen on the center key (see Roberts & Grant, 1974). It should be emphasized that these factors cannot account for the majority of the RI effects observed in this experiment. Significant RI effects appeared when patterns were the interpolated stimuli and either colored or patterned sample stimuli were used, and when colors were the interpolated stimuli and patterns were used as sample stimuli; in none of these cases did specific interpolated stimuli bias performance in favor of one sample stimulus over another.

Taken together, the data from this experiment suggest that there are at least two sources of RI in pigeon STM. One source is identified in the sense that the factors which produce it are known. These factors come into play whenever an interpolated stimulus is used which is more similar to the incorrect test stimulus than to the sample. The fact that robust RI effects appeared when no apparent intradimensional similarity effects were present indicates that a still unidentified source of RI was at work. The findings of this experiment have allowed us to rule out some candidates, that is, similarity of classes of stimuli, dimensional saliency, and familiarity.

EXPERIMENT 2

This experiment was designed to analyze systematically the dimensions of complexity and degree of emitted illumination as determinants of RI. Red and green fields were used as sample stimuli, and four black-and-white patterns were used as interpolated stimuli. The interpolated stimuli were chosen to yield a 2 × 2 factorial manipulation of complexity and illumination. Two of the interpolated stimuli were white crosses on a black background, and the other two were white vertical lines on a black background. One cross and one line were of small surface area, and other cross and line were of large surface area, being slightly greater than twice the surface area of the small figures. If complexity is an important determinant of RI, matching ought to be substantially lower with the crosses as interpolated stimuli than with the lines as interpolated stimuli. Greater RI effects for the large patterns than for the small ones would support an illumination factor, since the large patterns emit more illumination than the small ones.

Method

Subjects. Nine of the pigeons used in Experiment 1 served as subjects; 1 bird was not used because of generally poor matching performance during the first experiment. These animals were maintained at 80% of their free-feeding weight throughout the experiment.

Apparatus. The apparatus was the same as that used in Experiment 1, with one modification: Four black-and-white patterns not used before were constructed and photographed, and the resulting transparencies were inserted into the projectors mounted behind the center keys in each operant chamber. Two of the patterns were of a vertical white line (one large and one small) on a black background, and the other two patterns were of a white cross (one large, one small) on a black background. The surface area of the large patterns was 2.1 times greater than that of the small patterns. When projected on the center key, the small line measured 6.5 × 22.3 mm, and the large line measured 13.6 × 22.3 mm. The vertical and the horizontal bars of each cross each measured 22.3 mm in length, but the arms of the small cross were 3.6 mm in width and those of the large cross were 8.4 mm in width. The surface area of the small line and cross was 145 mm^2 and that of the large line and cross was 303.3 mm^2. The small patterns illuminated 27.3% of the surface area of the white key, and the large patterns illuminated 57.1% of the white key.

Procedure. The experiment was carried out over an 8-day period, with each bird tested for 48 trials on each day. On each trial, either a red or a green field was presented for 6 sec as the sample stimulus, and red and green side keys were presented as comparison stimuli. Four different sequences of 48 trials each were constructed. Each order contained 24 trials on which interpolated stimuli were presented during a 5-sec delay between termination of the sample stimulus and presentation of the comparison stimuli. A line pattern was presented as the interpolated stimulus on 12 of these trials, and a cross pattern was presented on the other 12 trials. Of the remaining 24 trials within a day, 12 trials were devoted to control tests on which a darkened key appeared during the delay, and 12 trials were 0-sec delay trials inserted to maintain a high level of performance. These four types of trials were presented in random sequences, with color and left-right position of the sample stimulus balanced across types of trials.

While the complexity variable was varied within days, degree of emitted illumination was changed between days. Thus, the line and cross patterns presented as interpolated stimuli on any given day were both small on every trial or both large on every trial. Small and large patterns were alternated between days, with five birds tested with small patterns on odd days and large patterns on even days, and the other four birds tested with the reverse assignment of pattern size to days. The daily sequences of trials were tested in different orders from bird to bird, with the requirement that small and large patterns of interpolated stimuli each be combined once with each sequence of trials.

Results

Table 2 shows the percentage of correct responses that were made on control trials and on trials with each of the four interpolated stimuli. Separate percentages are presented for trials with red and green sample stimuli (with each percentage based on 432 trials for the control condition and on 216 trials for each interpolated stimulus condition), as well as the mean percentages for each condition. An examination of the mean percentages indicates that matching performance with any one of the interpolated stimuli was inferior to that with the control. Further, both of the large patterns show a lower level of matching than the small patterns. Within each of the size categories, better performance is shown with the cross as an interpolated stimulus than with the line; this difference is negligible with small patterns and more marked with large patterns. Clearly, these data fail to support the notion that increased complexity increases the degree of RI. An ANOVA was performed to compare percentage of correct

TABLE 2

Percentage of Correct Responses for Interpolated Stimuli Varying in Complexity and in Amount of Illuminated Area as a Function of Color of Sample Stimuli

Sample stimuli	Control	Interpolated stimuli			
		Small line	Small cross	Large line	Large cross
Green	84.3	81.0	77.3	65.3	70.8
Red	84.0	80.6	85.2	76.4	84.3
M	84.1	80.8	81.2	70.8	77.6

responses across the four interpolated stimuli and the control condition; a significant effect of these conditions of interpolation was shown, $F(4, 32) = 7.03$, $p < .001$. Each interpolated stimulus was compared with the control condition using one-tailed t tests. Significant differences were found for the large line and large cross, $ts(8) = 3.23$ and 1.98, respectively, $p < .05$, but not for the small line and small cross, $ts(8) = 1.42$ and .88, respectively, $p > .05$. Thus, RI was found with the large patterns but not with the small ones. A Complexity × Area of Illumination × Subjects ANOVA was carried out using just the data from tests with interpolated stimuli and revealed significant effects of complexity, $F(1, 8) = 8.03$, $p < .025$, and of area of illumination, $F(1, 8) = 15.56$, $p < .01$, but a nonsignificant interaction of these variables, $F(1, 8) = 3.75$, $p > .05$.

A close look at Table 2 suggests that the color of the sample stimulus may have interacted with interpolated stimuli, as was the case for colored interpolated stimuli in Experiment 1. Specifically, the data suggest that pigeons could match more accurately when the sample was red than when it was green if the interpolated stimulus was a small cross, a large line, or a large cross but not when it was a small line or a darkened key (control trial). As a test of these possible effects, the difference in percentage of correct responses between green and red sample stimuli was compared between interpolated stimuli and the control condition. Two-tailed t tests were used since no directional predictions regarding biasing effects of this sort were made at the outset of the experiment. The small cross and large line clearly failed to differ significantly from the control, $ts(8) = 1.97$ and 1.70, respectively, $p > .05$; only the large cross approached a significant difference, $t(8) = 2.25$, $p \cong .06$.

Discussion

Experiment 2 was carried out in order to reveal the basis for the finding in Experiment 1 that a pattern of diagonal dots yielded less RI than colored fields or a pattern of crossed dots. Stimulus complexity and degree of emitted light were hypothesized to be possible relevant dimensions of interpolated stimuli which could produce this effect. The findings of Experiment 2 have ruled out complexity as a factor influencing RI but support the conclusion that degree of illumination is an important factor.

Two findings of this experiment are somewhat puzzling. One is the fact that line patterns produced more RI than the cross patterns; although this difference is directly opposite that predicted from the hypothesis that RI increases with complexity of the interpolated stimulus, the opposite effect now requires explanation. One possible explanation is based on the observation that the line patterns tended to concentrate the emitted illumination more toward the center of the key than did the more spread-out cross patterns. If the pigeons attended more to the center of the illuminated key than to the periphery, the greater effect of lines might be caused by the degree of illumination.

A second unexpected result was a tendency for certain interpolated stimuli to bias performance in favor of one sample stimulus over the other. Although only marginally supported statistically, the data suggest that presenting white light on the center key may lead to a preference for the red comparison stimulus over the green comparison stimulus. There are two ways in which such an effect can be interpreted. One explanation is to hold that these pigeons have a preexperimental preference for pecking red over green and that this preference was manifested whenever a bird could not remember the sample stimulus. Thus, on control trials, no strong preference should be seen since the sample was remembered frequently and formed the basis for response. However, with an effective inhibitory stimulus presented during the delay, memory of the sample was often absent and led birds to resort to a color preference. The other explanation is that advanced to account for the biasing effects of colored interpolated stimuli in Experiment 1. This account would have to argue that the pigeons perceived the interpolated white illumination as more similar to the red key than to the green key and therefore chose the red key as a result of stimulus generalization. The stronger such a process might operate, the closer performance would approach chance. To the extent that this kind of stimulus-generalization effect may have been operating in Experiment 2, it confounds the interpretation of the RI effects of illumination. That is, both a specific effect of stimulus generalization and a more general effect of illumination upon retention may have been involved, but the effects are not separated easily by examination of these data.

EXPERIMENT 3

This experiment was performed to establish more firmly the effects of center key illumination upon RI and to do so in a situation where center key illumination would not bias test responses in favor of one comparison stimulus over the other. The sample stimuli used were patterns of horizontal and vertical black-and-white stripes. An entirely illuminated center key was used as the interpolated stimulus, and its intensity was varied by introducing varying degrees of resistance into the center key circuit.

Method

Subjects. The nine birds used in Experiment 2 were the subjects in this experiment, being maintained at 80% of free-feeding weight.

Apparatus. The same basic apparatus as used for the first two experiments was used in Experiment 3. An Eico Resistance Decade Box was wired in series with the .1-A/28-V lamp mounted behind the center key which allowed the intensity of the center key illumination to be varied by changing the resistance.

Procedure. The birds were tested for 48 trials per day over two blocks of days, each block containing 8 days. On each trial, a pattern of either three vertical or three horizontal white stripes on a black background appeared for 8 sec on the center key as the sample stimulus. Vertical and horizontal stripes appeared on the side keys as comparison stimuli. Of the 48 daily trials, 24 trials were RI test trials on which an illuminated center key appeared during a 5-sec delay between termination of the sample stimulus and onset of the comparison stimuli, 12 were control trials on which the center key was darkened during the 5-sec delay, and the other 12 trials were 0-sec delay trials. These three types of trials were randomly intermixed with one another. Intensity of center key illumination was varied across 7 values of resistance, 0, 20, 40, 80, 160, 320, and 640 Ω. These values were chosen to provide a broad range of illumination, from the full intensity of the normally illuminated center key to what appeared to the human eye to be a very dimly illuminated key at 640 Ω. In addition, an 8th condition was run in which the center key was turned off as on control trials. Illumination was varied between days, with resistance set at a fixed level for the 24 RI test trials of any given day. Each bird was tested for 1 day at each intensity value within each of the two 8-day blocks. Four different sequences of 48 trials were used, and as far as possible these orders were combined equally often with intensity values across birds. The order in which the different values of intensity were tested varied from bird to bird in both blocks of days.

Results

The outcome of this experiment is shown in Figure 1, which plots matching accuracy on RI test trials against resistance. (Each point on the curve is based on 432 trials.) Except for one point on this curve, there is a monotonic increase in accuracy of matching as resistance increases. In fact, the curve closely approximates a linear function. An Intensity Level × Blocks of Days × Subjects ANOVA was carried out with these data and showed a significant effect of intensity, $F(7, 56) = 10.72$, $p < .001$, but no significant effect of blocks of days or of the interaction of these variables. The same ANOVA was carried out on the control trials data obtained on each day that a given intensity value was tested; this analysis showed no effects of intensity level, blocks of days, or their interaction, all $F\text{s} < 1$. The overall percentage of correct responses on control trials was 84.9, and this value is drawn as a horizontal broken line in Figure 1 to show the control level of accuracy. As resistance decreases and intensity of illumination increases the control and test curves progressively diverge, showing increasing RI with increasing center key brightness. Each intensity value was compared with the control level by t tests, and the RI effect was significant in each case ($p < .01$), except at the value of 640 Ω and at the off condition ($p > .05$). As a test for the possibility

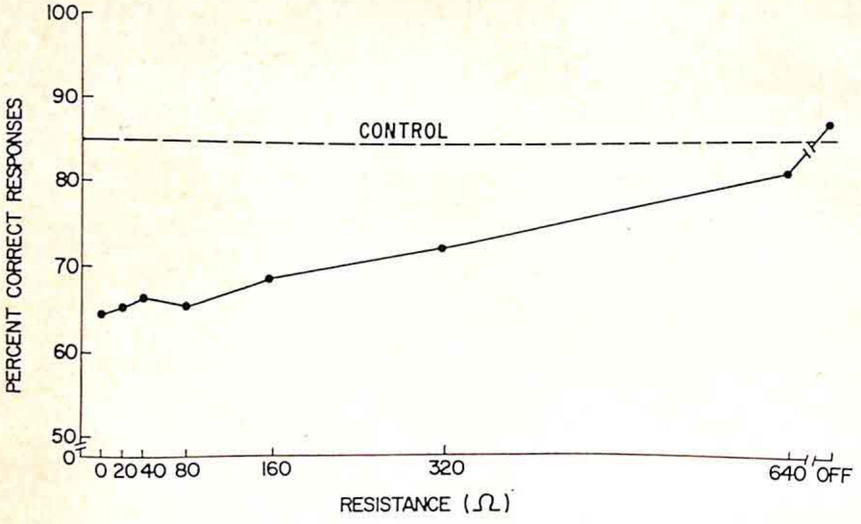

FIGURE 1. Matching accuracy on retroactive inhibition test trials plotted as a function of decreasing intensity of center key illumination (increasing resistance), with control accuracy show by the horizontal broken line.

that center key illumination biased the bird's choices in favor of one comparison stimulus over the other, difference scores were calculated by subtracting percentage of correct responses on trials when vertical stripes were the sample from percentage of correct responses on trials when horizontal stripes were the sample. The difference score for each intensity value was tested against that for the off condition, and it was found that none of these comparisons approached significance, all $ts < 1$. Thus, the strong RI effects found in this experiment appeared in the absence of any tendency for the interpolated stimulus to bias performance in favor of one comparison stimulus over the other.

EXPERIMENT 4

Experiment 3 has shown that a powerful inhibitory effect is exerted upon pigeon STM by illumination emitted from the center key. Other studies (see D'Amato, 1973) have shown that ambient illumination provided by a houselight during delay has the same effect upon monkey STM. In this experiment, RI test trials were run on which the center key remained darkened throughout a delay and the intensity of illumination provided by the houselight was varied. The purpose of the experiment was to determine whether a more ambient source of illumination would have the same effect upon retention as illumination emitted from the center key.

Method

The same subjects and apparatus used in Experiment 3 were used again in Experiment 4. The only modification was that the Eico Resistance Decade Box was placed in series with the houselight (.1-A/28-V lamp), which was mounted on the same wall as the pecking keys, 5 cm above the upper rim of the center key. The birds were tested in two blocks of 8 days each, with 48 trials given each day. The experimental design and procedures were exactly the same as those used in Experiment 3, except that the seven values of resistance used were 0, 40, 80, 160, 320, 640, and 1,280 Ω. The 0-Ω value provided full houselight illumination, whereas the 1,280-Ω value provided a very dim source of illumination, barely perceptible to the human eye. As in Experiment 3, an off Condition also was run as an eighth value along the intensity continuum.

Results

Figure 2 shows the effect of houselight intensity upon delayed matching accuracy. The effect is very similar to that seen in Figure 1 for center key illumination; as the houselight was dimmed (increased resistance), matching accuracy improved continuously. An ANOVA showed that the intensity factor was highly significant, $F(7,$

56) = 21.84, $p < .001$, but that neither blocks of days nor the interaction of Intensity × Blocks of Days was significant. An ANOVA on control data taken on days with different intensity values showed nonsignificant effects of intensity, blocks of days, and their interaction; overall control performance, 83.5%, is shown as the broken horizontal line in Figure 2. Percentage of correct responses made on RI tests at each intensity value was tested against the control, and significant RI effects were found ($p < .05$) at each value between 0 and 640 Ω, with only the 1,280-Ω and off conditions failing to differ from the control.

An examination of performance on trials with each sample stimulus revealed a consistent preference for horizontal stripes over vertical stripes. This preference was most marked at resistance levels 0–640 Ω and was less apparent at 1,280 Ω and off. However, t tests found that the horizontal stripes minus vertical stripes difference scores differed between the off condition and the various levels of intensity only at the 320-Ω level, $t(8) = 2.95$, $p < .02$. Since the possibility of stimulus generalization between houselight illumination and horizontal stripes seems remote, we interpret what biasing effects appeared in these data as a tendency for birds to fall back upon a preference for horizontal stripes when the sample stimulus cannot be remembered.

General Discussion

These experiments began with the intention of evaluating a numger of factors which might affect RI in pigeon STM. It appears now that there are two factors which produce RI, and only one of these can be classified as an effect upon retention. If the stimuli used in the matching task and the interpolated stimuli vary along the same dimension, then RI effects may result from stimulus generalization. This occurs when the interpolated stimulus is more similar to the incorrect comparison stimulus than to the sample stimulus. A good example of this effect was found in Experiment 1 when the sample was red, the interpolated stimulus was blue, and the incorrect comparison stimulus was green. Clearly, however, this kind of disruption of matching can occur without any forgetting of the sample stimulus.

A number of RI effects have been demonstrated in these experiments which cannot be attributed to stimulus generalization and which appear to involve loss of memory of the sample stimulus. We have tested the effects of dimensional saliency, similarity, familiarity, complexity, and degree of illumination as possible dimensions of inter-

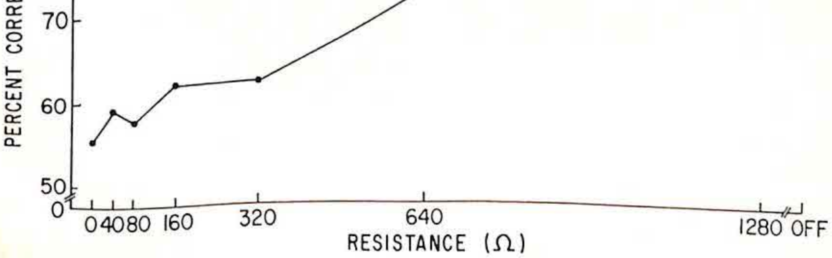

FIGURE 2. Matching accuracy on retroactive inhibition test trials as a function of decreasing intensity of houselight illumination (increasing resistance), with control accuracy shown by the horizontal broken line.

polated stimuli which might be responsible for these RI effects. Our experiments suggest little influence by any of these factors except illumination, which appears to have a strong effect upon retention. Further, the effects of illumination are equally powerful regardless of whether the illumination is from a specific source (the center key), or is ambient illumination (the houselight). The illumination factor can explain all of the effects observed in Experiment 1 reported here and the findings of Zentall's (1973) experiment. The greater RI effects found with blue and yellow fields and the crossed dots pattern than with the diagonal dots pattern or the vertical line used in Zentall's experiment can be explained as a result of more light emitted by the former interpolated stimuli than by the latter two interpolated stimuli.

That interpolated illumination has powerful effects upon short-term retention in the pigeon perhaps should be surprising, since a number of experiments with primates have revealed similar effects. Some of the primate literature on the effects of interpolated illumination on spatial delayed response and confounded visual–spatial delayed-response problems seems inconsistent, in that light has had both inhibitory and facilitatory effects on performance. (See D'Amato, 1973, and Etkin, 1972, for discussions of this literature.) It is clear, however, that with the DMTS procedure interpolated illumination interferes strongly with the retention of visual information in monkeys (D'Amato, 1973). The fact that the same inhibitory influence of illumination now has been discovered in pigeons, a nonmammalian species, gives this effect considerable comparative generality.

What could be the basis for this powerful influence of illumination upon retention? Several hypotheses have been offered to explain the effect with primates, and, unfortunately, most have been found wanting. It has been suggested that light causes animals to be more active and that activity inhibits retention. But Etkin (1972) found no difference in the activity of capuchin monkeys in light and dark and reported further that activity on trials when monkeys made a correct response did not differ from that on trials when they made an incorrect response. In addition, Jarrard and Moise (1970) found that stumptail monkeys who were allowed free movement in an operant chamber performed as well on DMTS as monkeys restrained in a primate chair.

Still another possibility is that sheer change in illumination, rather than light or dark per se, might have inhibitory or facilitatory effects on retention. However, D'Amato and O'Neill (1971) have shown that darkness leads to better retention than light, regardless of whether it is a constant condition or is introduced as a reduction in illumination. A third hypothesis is that the introduction of light has a destructive effect upon either an afterimage or a neural consolidation process. Assuming that either of these processes would be maximally susceptible to interference immediately after sample presentation, Etkin (1972) tested this hypothesis by inserting probe lights into the beginning and end of the delay period and found that point of interpolation had no effect on degree of RI. It appears that with monkeys, the memory of the sample stimulus is no more susceptible to interference at the outset of a delay period than it is later in the period.

D'Amato's (1973) interpretation of the illumination effect in monkeys has been integrated with his general theory of forgetting effects in DMTS. This theory proposes that monkeys forget not because of any decay or destruction of a memory trace but because the memory of a sample stimulus presented on a current trial is difficult to discriminate from the memory of a prior occurrence of that stimulus as an incorrect comparison stimulus on a preceding trial. The more distant in time the presentations of the sample took place, the more difficult is the temporal discrimination between the stimulus's occurrence as sample and as incorrect comparison stimulus. The inhibitory effect of illumination is held to occur because an illuminated delay, filled with visual stimulation, gives an animal the impression of a longer period of time than does a darkened delay. Thus, temporal discrimination is more difficult following an illuminated delay than following a dark-filled delay.

There are two difficulties with this hypothesis. Although there is evidence to show that temporal discrimination problems are undoubtedly one source of forgetting in monkey experiments (Mason & Wilson, 1974; Worsham & D'Amato, Note 1), attempts to produce differential forgetting by varying the ratio between time intervals (from the presentation of a stimulus as sample and its presentation as incorrect comparison stimulus) have not produced impressive results (Worsham & D'Amato, Nite 1). Worsham and D'Amato (Note 1) found that the delay interval within a DMTS trial accounted for about five times more forgetting than did the recency ratio. If direct manipulation of the possibilities for temporal discrimination produces relatively weak effects, it is difficult to see how this process could account for the dramatic effect of illumination. A second problem comes in the application of this theory to the illumination effect observed with pigeons. Data from an experiment by Grant (Note 2) have shown that occurrence of a given DMTS trial's sample stimulus as either the correct or incorrect comparison stimulus on trials preceding the current one by more than a few seconds has no effect on retention of the sample stimulus. Thus, temporal discrimination does not appear to be an important factor causing forgetting in pigeon DMTS experiments. (See Roberts and Grant, in press, for a more extended discussion of this issue.) Yet, strong effects of interpolated illumination are still observed.

Two further hypotheses may be offered to account for the illumination effect. One is that the memory trace of the sample stimulus is consolidated almost immediately after presentation of the sample, but that presentation of light acts to degrade or distort this trace in some fashion. Posner and Konick (1966) have called this the acid-bath model of forgetting. Another alternative is to make use of the rehearsal mechanism suggested by Wagner and his colleagues (Terry & Wagner, 1975; Wagner, Rudy, & Whitlow, 1973). A series of ingenious experiments using classical conditioning in rabbits has led this group to conclude that animals postperceptually process (rehearse) the events of a preceding trial, and that the extent of learning is directly related to the degree of rehearsal. Further, the extent to which an animal will rehearse is held to increase with the degree to which the events of a trial are surprising or unexpected. If a given trial's events are followed immediately by a surprising posttrial episode, rehearsal of that trial will be interfered with because processing time normally taken up by the trial's events will be usurped by rehearsal of the posttrial episode. This theory may be extended to DMTS by holding that pigeons remember the sample stimulus by rehearsing some representative of the sample stimulus throughout the delay. The illumination effect could be explained then by holding that light provides visual stimulation from the operant chamber which engages the rehearsal process and thus interferes with rehearsal and retention of the sample stimulus. It would have to be assumed that the rehearsal of the sample process is fairly constant throughout the delay in order to explain the fact that point of interpolation has no effect on degree of RI.

There is no strong evidence to support either of these latter two hypotheses, nor to negate them, Still other interpretations of the illumination effect may be available in addition to these. What seem called for now are experiments designed to reveal the basis of the illumination effect. A viable explanation of this effect may enrich our understanding of memory mechanisms in animals.

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Determinants of the Enhancement of Flavored-Water Intake by Prior Exposure

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The intake of a 2.0% sodium saccharin solution in rats was observed to increase as a function of both the number (Experiment 1) and the duration (Experiment 3) of prior periods of access to the saccharin flavor, but did not increase when subjects were maintained on a fluid deprivation procedure in the absence of saccharin exposure (Experiment 2). The enhancement of intake was further influenced by the schedule of saccharin preexposures in the absence of variations in the amount of solution tasted (Experiment 4). The effect was not a function of the opportunity for subjects to determine their own pattern of contact with the saccharin flavor, the opportunity for association of the flavor with hunger and thirst reduction, or the amount of saccharin swallowed during preexposure (Experiment 5). These results suggest that mere exposure to a flavored solution is sufficient to increase subsequent intakes. The phenomenon is discussed in terms of the attenuation of neophobia elicited by the novelty of flavored solutions.

There are few decisions animals make which are more closely related to their survival than the decision to eat something. Although ingestion of certain materials, such as sodium chloride, appears to be governed by innate recognition mechanisms (Krieckhous & Wolf, 1968; Stricker & Wilson, 1970), intake of many other substances is determined by the previous experience individuals have with these materials (Capretta, Moore, & Rossiter, 1973; Garcia, Hankins, & Rusiniak, 1974; Guthe, 1945; Revusky & Garcia, 1970; Rozin & Kalat, 1971). For example, the experience of a substance in conjunction with aversive stimulation decreases subsequent ingestion of this material (e.g., Richter, 1953; Rzoska, 1953), whereas association with positive stimulation tends to increase subsequent intakes (e.g., Zahorik, Maier, & Pies, 1974). In addition, prior exposure to edible materials in the absence of experimentally presented positive or negative stimulation can also affect subsequent ingestion. Exposure to edibles has been observed to increase later intake of these materials in rats (e.g., Barnett, 1956; Barnett & Spencer, 1953; Capretta & Rawls, 1974; Carroll, Dinc, Levy, & Smith, 1975; Domjan, 1975; Domjan & Bowman, 1974; Gentile, 1970; Green & Parker, 1975; Nachman & Jones, 1974; Navarick & Strouthes, 1969; Sheffield & Roby, 1950; Shortens, 1954; Siegel, 1974; Singh, 1974; Young, 1944), dogs (Maslow, 1932), guinea pigs (Warren & Pfaffmann, 1959), turtles (Burghardt & Hess, 1966), fish (Mackay, 1974; Miller, 1963), birds (Capretta, 1969; Capretta & Bronstein, 1967; Coppinger, 1969, 1970; Morell & Turner, 1970; Rabinowitch, 1968, 1969), and humans (Hollinger & Roberts, 1929).

Even though many investigators have observed increased intakes of edibles as a function of prior exposure, little systematic research has been done on the effect. The present study was designed to investigate the phenomenon in domesticated rats with a concentrated solution of sodium saccharin

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used as the ingested substance. Experiments were performed to determine the relationship between the intake of saccharin and the number and duration of prior daily exposures to the solution, how the increase in saccharin intake with experience affects the subjects' other ingestive activities, and the importance of approach and ingestive behaviors and thirst and hunger reduction in the intake enhancement effect.

EXPERIMENT 1

Experiment 1 was designed to determine the relationship between saccharin intake and the number of prior daily exposures to the solution as well as how changes in saccharin intake are related to the ingestion of other substances.

Method

Twenty-three 200–250-g male Sprague–Dawley rats (Holtzman, Madison, Wisconsin) were individually housed with continual access to Purina Rat Chow and tap water and remained in their home cages throughout the experiment. Several days after arrival in the laboratory, subjects were restricted to a daily 23.5-hr. water deprivation schedule. After having been on this schedule for 3 days, they were assigned to one of two groups. One group (Group S, $n = 16$) received access to a 2.0% solution (W/V in tap water) of sodium saccharin for 30 min. immediately before the daily 30 min. access to water, while the other group (Group W, $n = 7$) continued to receive access to only water for 30 min. each day. These procedures were continued for 20 consecutive days. Fluids were presented in graduated drinking tubes, and intakes were recorded to the nearest .5 ml. All two-group comparisons were made with the Mann–Whitney U test (two-tailed), and within-subject comparisons were made with the sign test (two-tailed).

Results and Discussion

The mean daily fluid intakes of Groups S and W are presented in Figure 1. Initially, Group S drank a mean of only 1.3 ml of saccharin in 30 min. in contrast to 13.7 ml of water ingested by Group W during a comparable period ($U = 0$, $p < .01$). This initial reluctance of subjects in Group S to ingest the concentrated saccharin solution was noteworthy not only because of the low levels of intake but also because subjects appeared to become aroused in their attempts to drink. Although no systematic measurements of behavior other than drinking were made, subjects were observed to make repeated approach and withdrawal responses to the drinking tube and often rattled and bit parts of the cage. Such agitation in response to a novel edible substance has also been observed in Burmese jungle fowl chicks by Hogan (1965).

Increased exposure to the saccharin flavor with repeated testing led to gradual increases in intake of the saccharin solution. All subjects in Group S drank more saccharin on Day 2 than Day 1, and their mean intakes increased in a negatively accelerated fashion until Group S drank as much saccharin as Group W did water. As saccharin intakes increased, the approach and withdrawal behaviors as well as the "displacement" rattling and biting responses declined.

As might be expected, the amount of water the subjects in Group S drank each day immediately following their exposure to saccharin was inversely related to their saccharin intake. Figure 1 shows that as saccharin intakes increased water intakes in Groups S declined. Such an inverse relationship between gradually increasing in-

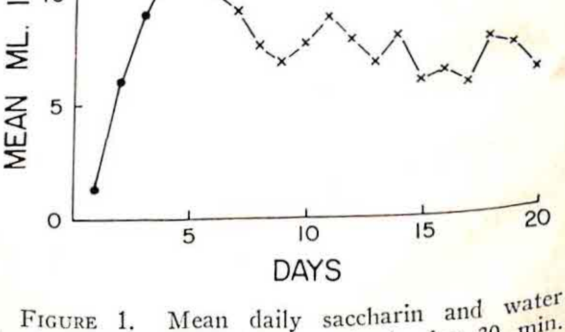

FIGURE 1. Mean daily saccharin and water intakes of subjects given saccharin for 30 min. followed by water for 30 min. each day (Group S), and mean daily water intakes of subjects given only water for 30 min. each day (Group W).

takes of a flavored solution and gradually declining intakes of water was also observed by Strouthes and his associates (Navarick & Strouthes, 1969; Strouthes, 1971) when they gave rats 90 min. daily access to a choice between water and a mixture of sodium saccharin and soda bicarbonate.

Although the intake of water in Group S declined as saccharin intakes increased, Group S continued to drink more than a mean of 6 ml of water throughout the experiment. Therefore, toward the end of the study, the daily fluid intakes of Group S exceeded the total fluid intakes of Group W. For example, on Day 20, Group S drank a mean total of 23.4 ml, whereas Group W drank only 15.6 ml ($U = 0, p < .01$). This difference was probably not a result of the longer total daily durations of access to fluids in Group S (60 min.) as compared to Group W (30 min.) since in replications of the present experiment we observed Group S drink more than Group W even when the fluid access periods for Group W were extended to 60 min. Group S may have ingested more than Group W because it was exposed to two solutions (saccharin and water) rather than just one (water), and therefore its intakes were not as suppressed by the temporary decrement in palatability which occurs with exposure to a single taste (Holman, 1973; Morrison, 1974). Alternatively, Group S may have ingested more than Group W because the greater tonicity of 2% saccharin as compared to tap water delayed the activation of cellular hydration inhibitory mechanisms for drinking in Group S (Blass & Hall, 1974; Stricker, 1969, 1971).

The procedure of giving Group S access to water for 30 min. each day following the presentation of saccharin insured that subjects could satisfy their daily fluid needs even if they chose not to drink much saccharin. The inverse relationship obtained between saccharin and water intakes in Group S suggests that subjects did in fact take advantage of this aspect of the procedure. Therefore, the gradual increase in saccharin intakes across successive days was probably not a result of increased levels of thirst developing across successive days in Group S.

EXPERIMENT 2

The results of Experiment 1 indicate that intakes of a relatively high concentration of saccharin gradually increase as subjects have increased exposure to the flavor. However, the daily saccharin presentations in Experiment 1 were confounded with increased exposure to the laboratory environment and the water deprivation schedule. Experiment 2 was designed to determine whether these factors may have been responsible for the enhancement of saccharin intake observed in Experiment 1. One group of subjects, like Group S of Experiment 1, received saccharin on each of 7 consecutive days, while another group was maintained on the water deprivation schedule without access to saccharin for 17 days between its second and third exposure to the saccharin flavor. Maintenance on the deprivation schedule without saccharin for 17 days was expected to enhance subsequent saccharin intakes in this latter group if increased exposure to the laboratory environment and the deprivation schedule was responsible for the enhancement of saccharin intake observed in Experiment 1.

Method

The method was identical to that of Experiment 1 in all unspecified details. After adaptation to the housing conditions and the water deprivation schedule Group U (uninterrupted, $n = 8$) received access to a 2.0% sodium saccharin solution for 30 min. on each of seven consecutive days. Group I (interrupted, $n = 8$) experienced a 17-day interruption between its second and third test with saccharin. During this interruption period, the only fluid these subjects received was water for 60 min. daily.

Results and Discussion

The amount of saccharin ingested by each group during each of the seven 30 min. test sessions is shown in Figure 2. As in Experiment 1, subjects initially drank very little of the saccharin solution but gradually increased their intakes with repeated exposure. Furthermore, Group U and Group I drank similar amounts of saccharin during

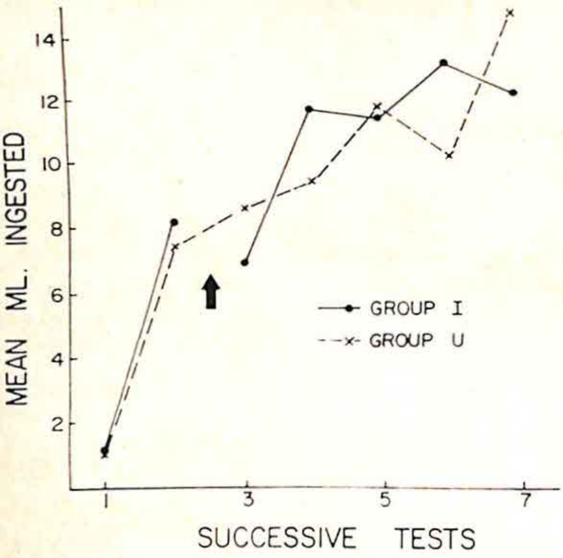

FIGURE 2. Mean saccharin intake during successive 30-min. tests for subjects tested on 7 consecutive days (Group U) as compared to subjects whose test sequence was interrupted, as indicated by arrow, for 17 days between Tests 2 and 3 (Group I).

each test session despite the fact that subjects in Group I suffered a 17-day interruption between Tests 2 and 3. These results suggest that increases in saccharin intake are primarily a function of the number of prior experiences with the solution rather than exposure to either the laboratory environment or the water deprivation schedule. The present findings also indicate that increases in the intake of a relatively novel solution are not attenuated by a retention interval of 17 days.

EXPERIMENT 3

In Experiments 1 and 2, the enhancement of flavored-water intake was primarily a result of the number of prior experiences with the flavor. Experiment 3 was designed to determine whether saccharin intake is also related to the duration of prior exposures to the solution.

Method

Eight Sprague-Dawley rats were assigned to each of five groups (0, 5, 30, 120, and 240) and maintained on a 23.5-hr. water deprivation schedule for the first 5 days. In the home cage on Days 6 and 7, before the 30 min. access to tap water, Groups 0, 5, 30, 120, and 240 received access to the 2.0% sodium saccharin solution for 0, 5, 30, 120, or 240 min., respectively. Only water was presented for 30 min. on Day 8, and on Day 9 each group received access to the saccharin solution for 30 min. following the 23.5-hr. water deprivation. The data were analyzed with the Mann–Whitney *U* test and the Kruskal–Wallis analysis of variance.

Results and Discussion

As expected, subjects with longer periods of access to the saccharin solution on Days 6 and 7 also drank more. The mean total saccharin intakes of Groups 5, 30, 120, and 240 during these 2 days were 3.4, 7.2, 24.6, and 34.9 ml, respectively ($H = 25.6$, $p < .01$).

The amount of saccharin ingested by each group during the 30 min. test on Day 9 following differential prior exposure to the saccharin flavor is presented in Figure 3. Each of Groups 5, 30, 120, and 240 given prior exposure to saccharin drank more during the test session than Group 0 which had not tasted the saccharin solution before the test (all Us < 4, ps $< .01$). This result indicates that as little as 5-min. exposure to a novel solution on two occasions is sufficient to elevate subsequent intakes of the substance.

Comparison of the amounts ingested by Groups 5, 30, 120, and 240 during the test session on Day 9 revealed a direct relationship between the duration of prior exposure to saccharin and the amount of subsequent

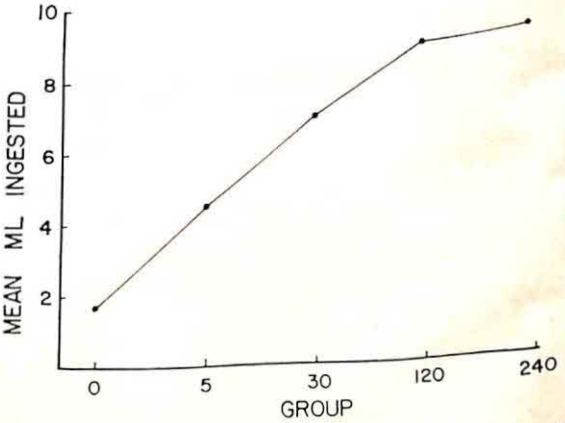

FIGURE 3. Mean intake of saccharin during a 30-min. test for independent groups that received access to the saccharin solution on each of two previous occasions for 0, 5, 30, 120, or 240 min.

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saccharin intake ($H = 10.1$, $p < .02$). This finding shows that ingestion of a flavored solution is determined not only by the presence or absence of prior exposure to the flavor but also by the duration of this prior access to the solution.

EXPERIMENT 4

Experiments 1, 2, and 3 indicated that intake of a 2.0% saccharin solution is directly related to the amount of prior experience subjects have with the flavor. During the exposure phase of these experiments, the saccharin solution was presented in drinking tubes which permitted each subject to determine its unique pattern of contact with the flavor. As was noted earlier, this method of stimulus presentation led to a great deal of approach and withdrawal behavior directed toward the drinking tube, particularly when the saccharin solution was novel. Experiment 4 was designed to determine whether exposure to saccharin can elevate subsequent intakes of the solution if subjects do not have the opportunity to determine their pattern of contact with the flavor during the exposure phase. In order to expose subjects to the saccharin flavor in the absence of approach behaviors, the solution was infused directly into the oral cavity through a fistula. The pattern of contact with the flavor was therefore fixed by the infusion schedule and was not influenced by the behavior of the subjects.

An additional objective of Experiment 4 was to clarify the factors responsible for the direct relationship obtained in Experiment 3 between the duration of access to saccharin and the amount of subsequent saccharin intake. Subjects given longer periods of access to saccharin before the test in Experiment 3 also drank more of the solution during the exposure phase. Therefore, differences in the duration of pretest exposure to saccharin were confounded with differences in the amount of saccharin solution passing through the oral cavity. This confounding was eliminated in Experiment 4 by using three different infusion schedules to manipulate the duration of exposure to the same amount of saccharin (12 ml) in three independent groups.

Method

Before the start of the experimental procedures, 27 male Sprague–Dawley rats were anesthetized with ether and implanted with an oral fistula similar to the one used by Domjan and Wilson (1972). Briefly, a section of polyethylene tubing (Clay–Adams P.E. 205) was passed under the skin, with one end exiting at the back of the neck and the other exiting just anterior to the right molar teeth. The two ends were flared and held in place by polyethylene washers and by a wire suture attached to the oral end and affixed to subcutaneous tissue in the cheek.

Access to water was restricted to 30 min. each day after subjects recovered from the operation. Four days after the start of the water deprivation schedule, each subject was assigned to one of three groups and adapted to the oral infusion procedure. Just before their daily 30 min. of water in the home cage, subjects underwent an adaptation session (three, in all) during which 12 ml of water were infused into their oral cavities. The infusion cages had tall walls with the cannulas attached via flexible rubber tubing to a syringe in a Harvard Model 941 infusion pump. Group 120 ($n = 9$) received a 1-ml infusion of water 12 times at 10-min. intervals, with the infusion pump set to produce a flow rate of .6 ml/min. Group 60 ($n = 9$) received six 2-ml infusions of water at 10-min. intervals with the infusion pump set at 1.2 ml/min, while Group 15 ($n = 9$) received two 6-ml infusions separated by 10 min. with the pump set at 2.9 ml/min. Adaptation sessions were conducted on alternate days.

Two days after the last adaptation session each subjects was exposed to the taste of a 2.0% sodium saccharin solution by getting the substance infused directly into the oral cavity on the same infusion schedule used during adaptation. Therefore, Group 120 was exposed to the saccharin flavor over approximately a 120-min. period, Group 60 received the saccharin over approximately a 60-min. period, and Group 15 received the oral infusions over about a 15-min. period. Subjects were allowed their daily 30 min. of water 1 hr. after the end of the infusions. Starting 2 days later, three daily free-ingestion test sessions were conducted during which subjects were allowed access to the 2.0% saccharin solution for 30 min. in the home cage immediately preceding access to water for 30 min. The data were analyzed with the Kruskal-Wallis analysis of variance and the Mann–Whitney U test (two-tailed).

Results and Discussion

Subjects appeared to find oral infusions of the 2.0% saccharin solution aversive, and some attempted, unsuccessfully, to escape

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by jumping out of the infusion cages. Such attempted escape responses were more evident in Group 120 than Groups 15 and 60, probably because Group 120 received more saccharin infusions. The aversiveness of the infusions was also evident in that subjects typically allowed some of the infused saccharin to flow out of their mouth instead of swallowing it.

The amount of saccharin ingested by each group during the 30-min. free-ingestion tests in the home cage is displayed in Figure 4. In Experiments 1, 2, and 3 the mean intake of saccharin during a 30-min period was always less than 2 ml for subjects that did not have prior exposure to the solution. In contrast, in the present experiment the mean intake during the first 30-min. free-ingestion saccharin test was greater than 2 ml for each group. Each group in Experiment 4 drank reliably more saccharin during Test 1 than Group 0 of Experiment 3, for example, which was tested without prior exposure to the flavor (all Us < 9, all ps $<$ .02). These results suggest, in agreement with Siegel (1974), that exposure to a flavored solution can enhance subsequent intakes of the substance even if this exposure does not allow subjects to determine their own unique pattern of contact with the solution. Therefore, the prominent approach–avoidance behaviors observed during the initial access to a flavored solution appear not to be necessary for this initial exposure to elevate subsequent intakes.

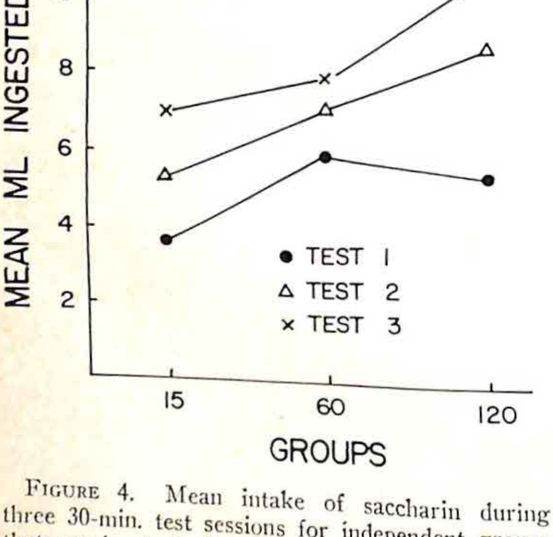

FIGURE 4. Mean intake of saccharin during three 30-min. test sessions for independent groups that previously had the saccharin solution infused into the oral cavity through a fistula over a 15-, 60-, or 120-min. period.

A relationship was not observed between amount of saccharin ingested during Test 1 and the extent of previous exposure to the solution ($H = 4.38$, $p > .10$). However, during both Tests 2 and 3 subjects that had received the saccharin infused into the oral cavity over a more extended period tended to drink more of the solution (Test 2: $H = 6.23$, $p < .05$; Test 3: $H = 7.56, p < .05$). Furthermore, during each of these tests Group 120 drank reliably more than Group 15 (Test 2: $U = 11.5$, $p < .02$; Test 3: $U = 12$, $p < .02$).

Since variations in the extent of preexposure to saccharin were accomplished by manipulation of the number and rate of the oral infusions as well as the quantity of each infusion, it is not clear which of these variables was responsible for the results obtained. Nevertheless, the present findings indicate that intakes of a flavored solution can be influenced by the schedule of prior exposures to the flavor even in the absence of variations in the amount of solution tasted.

It is noteworthy that the relationship between amount of saccharin ingested during the tests and the extent of prior exposure through oral infusions was not observed until Test 2. As was noted earlier, the oral infusions of saccharin appeared to be aversive, with the aversiveness probably most severe for Group 120, which was exposed to the infusion procedure for the longest duration. Perhaps the aversiveness of the infusion procedure conditioned a mild aversion to 2% saccharin in Group 120, and this aversion had to be extinguished before the facilitatory effects of extended prior exposure to saccharin could become evident during the free-ingestion tests.

EXPERIMENT 5

The results of Experiment 4 indicate that approach and withdrawal responses directed toward a novel flavored solution are not necessary for the enhancement of intake by prior exposure. Nevertheless, such approach–avoidance behaviors may facilitate the intake enhancement effect. Experiment 5 was designed in part to evaluate this possibility by comparing the saccharin intakes of subjects previously exposed to saccharin through oral infusions to the intakes of subjects that previously drank the solution from a drinking tube.

An additional objective of Experiment 5 was to determine whether exposure to a high concentration of saccharin can elevate subsequent intakes even if this exposure occurs when subjects are neither thirsty nor hungry. In Experiments 1, 2, 3, and 4 subjects were always water deprived during their exposure to the 2.0% saccharin solution and received their daily 30-min. access to tap water shortly afterward. Since their food source was dry pellets of Purina Chow, much of their daily food intake was probably also limited to the periods of saccharin and water availability. Therefore, the taste of the saccharin solution could have become a conditioned reinforcer through association with thirst and hunger reduction (e.g., Revusky, 1967, 1968, 1974), and this conditioning may have been responsible for the enhancement of intake in saccharin-preexposed subjects. This possibility was evaluated by comparing the saccharin intakes of subjects preexposed to saccharin either before or after access to food and water. Since the nondeprived subjects probably would not have ingested much saccharin presented in a drinking tube, the solution was infused into the oral cavity with the fistula technique used in Experiment 4.

Method

Forty male Sprague-Dawley rats, like the subjects in Experiment 4, had oral fistulas implanted in their cheeks. Upon recovery from the operation, they were placed on a daily 23.5-hr. water deprivation schedule. Sessions of adaptation to the oral infusion procedure began after subjects had been on the deprivation schedule for 5 to 6 days and were conducted on alternate days immediately before the daily 30 min. access to tap water in the home cage. During each adaptation session subjects received a 1-ml infusion of water 13 times at 12.5-min. intervals with the infusion pump set to produce a flow rate of .75 ml/min. This adaptation procedure was conducted for six sessions, rather than the three sessions used in Experiment 4, in order to further reduce the aversiveness of the oral infusions.

Following adaptation, each subject received one of five different types of exposure to the 2.0% saccharin solution. Group Si–D (saccharin infused–deprived, $n = 8$) received the saccharin by oral infusion following the 23.5-hr. water deprivation. The infusion schedule used was the same as that employed during adaptation. At the end of the infusions, any saccharin that may have gotten on the subjects' fur or paws was dried off with a soft paper towel, and subjects received access to water in the home cage for 1 hr. Group Si–ND (saccharin infused–nondeprived, $n = 8$) received the same type of exposure to saccharin as Group Si–D, except that its 1-hr. access to water in the home cage on the saccharin-infusion day occurred starting 2 hr. before the infusion. Therefore, in contrast to Group Si–D, Group Si–ND was exposed to saccharin after it had satisfied its hunger and thirst. Group Wi (water infused, $n = 8$) served as a control group. Instead of getting saccharin infused into the oral cavity, Group Wi received oral infusions of tap water, with the infusions conducted before the day's 1-hr. access to tap water for four subjects and afterward for the remaining four subjects.

The additional 16 subjects were exposed to saccharin by allowing them free ingestion (FI) of the solution from a drinking tube following the 23.5-hr. water deprivation. Group FI-home ($n = 8$) was allowed to drink the solution in the home cage, while Group FI-app ($n = 8$) was allowed to drink the solution in the infusion apparatus. For each free-ingestion group, the saccharin solution was available for 151 min. which is as long as the infusion procedure lasted for the infused groups (Si–D, Si–ND, and Wi). Since the infused groups did not have food available during the infusion procedure, food was also unavailable for the free-ingestion groups. Food was returned ad lib after the saccharin ingestion period, and subjects received access to tap water for 1 hr.

The day after the saccharin-exposure treatment, each group received access to water for 30 min., and a 60-min. free-ingestion test with the 2.0% saccharin solution was conducted the next day in the home cages following 23.5-hr water deprivation. Intakes during this test were recorded at 10-min. intervals and were evaluated with the Mann-Whitney U test (two-tailed).

Results and Discussion

Both Group FI–home and FI–app drank a mean of 12.0 ml of saccharin during the 151-min. period of access to the saccharin prior to the test session. This amount was not significantly different from the 13 ml of saccharin infused into the oral cavity of Groups Si–D and Si–ND ($t = .90$). Therefore, the various groups exposed to saccharin prior to the test session differed only in the way the saccharin was presented and not in the amount of solution experienced.

The amount of saccharin ingested by each group during the 60-min. post-exposure test in the home cage is presented in Figure 5. As expected, Group Wi, which had not tasted the saccharin solution before this test session, drank minimal amounts, reaching a mean total of only 3.6 ml during the 60-min. test. In contrast, each of the other groups, which had received some prior exposure to saccharin, drank substantially greater quantities during the test session (all Us < 4, ps $< .01$). Furthermore, the ways the various groups were previously exposed to saccharin appeared to be inconsequential. None of the differences in saccharin intake during the test session among Groups Si–D, Si–ND, FI-home, and FI–app were statistically significant.

The finding that exposure to saccharin through oral infusions increased subsequent intakes of the solution indicates, in agreement with Experiment 4 and Siegel (1974), that the approach-avoidance responses subjects typically exhibit when a solution is first presented are not necessary for this initial exposure to elevate subsequent intakes. The present results also suggest that such approach–avoidance behaviors do not enhance the effects of flavor exposure since the free-ingestion groups did not drink more saccharin during the test session than the groups that had received prior exposure to saccharin through oral infusions.

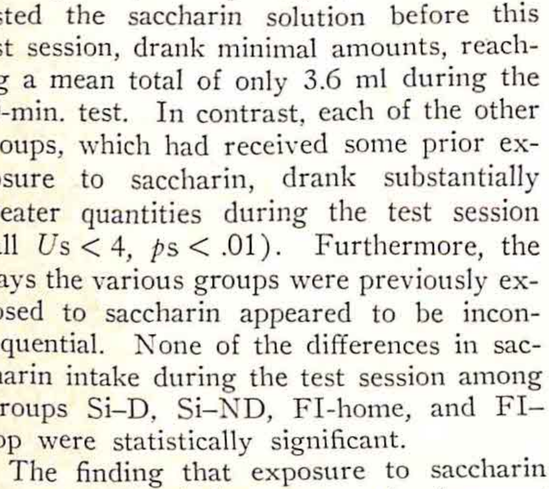

FIGURE 5. Mean cumulative intake of saccharin during a 60-min. test for independent groups that previously had either no prior exposure to the saccharin solution (Group Wi); prior exposure to saccharin through free ingestion in the infusion apparatus (Group FI-app) or in the home cage (Group FI-home); or prior exposure to saccharin through infusions into the oral cavity while water deprived (Group Si-D) or in the absence of deprivation (Group Si-ND).

Group Si–ND was exposed to saccharin in a manner that precluded the association of the taste of saccharin with hunger and thirst reduction. The fact that they drank more saccharin during the subsequent test than Group Wi indicates, in agreement with Siegel (1974), that the association of a taste with hunger and thirst reduction is not necessary for the enhancement of ingestion by prior exposure. The comparable test-session saccharin intake of Groups Si–D and Si–ND further suggests that the opportunity to associate the taste of saccharin with hunger and thirst reduction does not facilitate the enhancement of intakes by prior exposure in this situation.

The present results also suggest that the amount of saccharin swallowed during preexposure does not greatly influence the subsequent enhancement of saccharin intake. Although the amount of the infused solution that subjects swallowed and the amount they allowed to flow out of their mouth was not measured, subjects typically did not ingest all of the saccharin that was infused into the oral cavity. This was particularly true for Group Si–ND, which was preexposed to saccharin in the absence of deprivation. Nevertheless, the intake-enhancement effect in Group Si–ND was comparable to the effect in the free-ingestion groups (FI–home and FI–app) which swallowed all of the saccharin they tasted during preexposure.

GENERAL DISCUSSION

Since sources of food often also contain toxic materials, it is not surprising that a variety of species have evolved to ingest only small quantities of novel substances. In fact, such neophobia for edibles would be expected in all species that rely at least in part on experience rather than innate preferences to guide their food selection.

Having ingested a small quantity of a novel food and found it not to have aversive postingestional effects, animals gradually increase their subsequent intakes. The present experiments have shown that this enhancement of intake by prior exposure is a negatively accelerating function of both the number and duration of prior exposures, and is influenced by the schedule of taste exposures even in the absence of variations in the amount of solution tasted. However, under the conditions of the present experiments, the intake-enhancement effect does appear to be determined by either the opportunity for subjects to determine their own unique pattern of contact with the saccharin flavor, the opportunity for association of the flavor with hunger and thirst reduction, or the amount of saccharin swallowed during preexposure.

The fact that the intake-enhancement effect was primarily a function of the schedule of prior exposures to the flavor and occurred in the absence of both approach–avoidance behaviors and the association of the novel flavor with thirst and hunger reduction indicates that mere exposure to the saccharin flavor was sufficient to increase subsequent ingestion. Previous investigators have suggested that mere exposure may enhance subsequent intakes of a solution because it permits subjects to learn that the solution is safe to drink (Kalat & Rozin, 1973; Nachman & Jones, 1974; Siegel, 1974). However, if mere exposure produces learned safety, it should facilitate the conditioning of a stimulus as a safety signal. This does not seem to be the case with either taste or auditory cues (Best, 1975; Rescorla, 1971).

An alternative possibility is that mere exposure elevates subsequent intakes of a solution because it reduces the novelty of the substance and thereby attenuates the neophobia or aversion responses elicited by novelty. This explanation seems to be the most promising at the present time.

Mere exposure increases approach or positive responses not only to taste cues but also to a variety of other types of stimuli. For example, young chickens are more likely to approach familiar than novel objects (e.g., Zajonc, Reimer, & Hausser, 1973), rats and mice are more likely to eat in familiar than novel situations (Barnett, 1956, 1958; Chitty & Shortens, 1946; Jennings & McCutcheon, 1974; Mitchell, Kirschbaum, & Perry, 1975; Price, 1972; Shortens, 1954), and humans are more likely to give positive ratings to stimuli such as ideograms, musical excerpts, words, and pictures of people that they have been preexposed to (e.g., Hamm, Baum, & Nikels, 1974; Heingartner & Hall, 1974; Zajonc, 1968; Zajonc, Swap, Harrison, & Roberts, 1971). Whether a common mechanism is responsible for all of these phenomena remains to be seen. However, it is striking that all of them are produced by the same type of experimental manipulation. Therefore, it is possible that all of these effects involve in some way the attenuation of negative responses elicited by the novelty of a stimulus.

Although the explanation of exposure effects in terms of the attenuation of neophobia assumes that novelty elicits negative responses, it is important to note that this assumption does not imply that all novel stimuli will elicit neophobia. Since unfamiliar stimuli have a variety of features in addition to novelty, these other features either individually or in combination with novelty may control approach response tendencies.

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Modification of the Rat's Acoustic Startle Response by Antecedent Visual Stimulation

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Male hooded and albino rats were exposed to a light flash followed at various temporal intervals by a startle-eliciting 117 db. (re 20 $\mu N/m^2$) burst of white noise. The visual stimulus engendered startle response inhibition (maximally when the lead time was 64–250 msec) as well as startle response latency reduction (maximally when the lead time was 2–8 msec). The temporal functions for the effects of visual stimuli paralleled those previously reported for startle modification by acoustic events. Further study revealed that, given optimal lead times, inhibition is produced reliably by weaker visual stimuli (3×10^{-6} cd·sec/cm^2) than latency reduction (3×10^{-4} cd·sec/cm^2). This differential sensitivity to visual stimuli is also analogous to previously reported findings for events in the acoustic environment. It reveals that the neural mechanisms that mediate latency reduction and inhibition can be engaged by either acoustic or visual stimulation.

With the exception of the pioneering work by Landis and Hunt (1939) which examined startle in a variety of species using a variety of eliciting stimuli, the experimental analysis of the startle reaction has focused on the manner in which various events in the acoustic environment influence the rat's overt reaction to intense startle-eliciting acoustic signals (Hoffman & Searle, 1965, 1968; Hoffman & Wible, 1969, 1970; Marsh, Hoffman, Stitt, & Schwartz, 1975; Stitt, Hoffman, & Marsh, 1973; Stitt, Hoffman, Marsh, & Boskoff, 1974). Recently, however, Ison and his colleagues (Buckland, Buckland, Jamieson, & Ison, 1969; Ison & Hammond, 1971) have reported that events in the visual environment can also influence acoustically evoked startle. They found that a flash of light approximately 100 msec prior to an intense burst of sound will inhibit the rat's startle response to that sound and that the amount of inhibition diminishes with increasing time between the light flash and the startle stimulus. These results parallel earlier findings (Hoffman & Searle, 1965) that the rat's startle response to an intense acoustic signal is inhibited when a weak sound (which does not itself evoke startle) precedes the intense signal by 15–2,000 msec. The research reported here was designed to examine the effects of visual stimuli in greater detail.

The initial phase of the present research represented a replication of Ison's basic experiments. The next step was to expand on them by exploring various other ways that events in the visual environment might influence the rat's startle reaction. For example, Hoffman and Searle (1968) found that relatively weak acoustic stimuli delivered less than 10 msec before an intense startle-eliciting burst of noise reduced the latency of the startle response without affecting response amplitude. The work reported here examined this effect when a light flash rather than an acoustic signal preceded a startle-eliciting stimulus by less than 10 msec.

The final phase of the research examined the sensitivity of the startle system to small changes in the visual environment. In particular, the work sought the minimal intensity of light flash that might engender the

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inhibitory and latency shift effects. Marsh et al. (1975) examined small abrupt changes in the acoustic environment preceding a startle stimulus. They found the inhibitory phenomenon to be sensitive to small changes in either the frequency characteristics or the overall intensity of the antecedent acoustic background. On the other hand, the latency reduction phenomenon, while sensitive to small changes in frequency, was insensitive to the same intensity changes that were found to engage the inhibitory phenomenon. This led Marsh et al. (1975) to hypothesize that the effects represent the action of two different mechanisms. Data on the thresholds for visually engendered inhibition and latency shift would enable us to determine whether or not the two startle modification mechanisms differ in sensitivity, regardless of sensory modality.

EXPERIMENT 1

The goal of Experiment 1 was to examine and compare the rat's responsiveness to intense acoustic and visual stimuli.

Method

Subjects. Five experimentally naive male hooded rats supplied by Charles River Co. were used. They were approximately 170–180 days old at the time of testing.

Apparatus. The research was conducted in an Industrial Acoustics Corp. ventilated double-wall isolation room. This facility has an ambient noise level less than 20 db. for frequencies above 100 Hz. (All intensities given here are sound pressure level re 20 $\mu N/m^2$.)

The device for the assessment of startle consisted of a rigid superstructure that supported an animal chamber (20.3 × 15.2 cm) that was constructed of stainless-steel rods. The chamber was suspended from the superstructure by flexible plates of laminated fiberglass and epoxy. The suspension system was relatively stiff so that even vigorous movements of the subject produced displacements of the chamber which were so small that they could not be detected visually, and oscillatory movements were heavily damped.

The response detection system involved the transduction of chamber movements into electrical current and the measurement of the current on a storage oscilloscope. An aluminum rod which had a magnet at the end was firmly fixed to the top of the animal chamber. The magnet rode in a coil which was mounted rigidly on the superstructure. Movements of the magnet in the coil produced a current which was proportional to the *rate* at which magnetic lines of flux crossed the coil. For this reason, the device was highly sensitive to the sudden sharp movements involved in startle, but was relatively insensitive to the slow, though perhaps gross, movements of general activity. The output of the coil was filtered (to remove high-frequency artifacts resulting from the impact of intense acoustic signals), amplified, and passed to one channel of a Tektronix storage oscilloscope (Type 564).

Acoustic stimuli were generated by feeding the output of a General Radio random-noise generator (Type 1390-B) to a Grason-Stadler electronic switch (Model 829-C), operated by a Grason-Stadler interval timer (Model 471-1). This arrangement permitted control over the duration of the signals as well as their rise–decay time. A final stage of amplification, provided by a Crown DC 300 audio power amplifier, was employed before feeding the signals through a semiconductor gate to a speaker consisting of an Altec 230 C midrange driver with a 60-cm exponential horn. The semiconductor gate was a General Electric X12 Triac which appears as an open circuit until triggered and then permits current flow in both directions. The triggering circuit was such that the acoustic stimulus would fire the Triac, but the unwanted internal noise of the equipment would not. The intensity of the acoustic stimulus was assessed with a Hewlett-Packard one-half inch (17.7-mm) calibrated condenser microphone and General Radio 1561-A sound level meter. The sound field was sampled at several points inside the cage and the median intensity was taken as the nominal value.

Visual stimuli were produced by Argus electronic flash units (Model E-3). Two flash units were used because the interpulse interval used in the experiments was less than the recharging time of the units. Also, it was crucial that the light be presented silently, since even weak acoustic signals have been found to inhibit the response to a subsequent startle-eliciting stimulus (Hoffman & Wible, 1970). This posed a special problem because the flash units themselves produced a distinct "pop" when fired. In order to prevent subjects from hearing the "pop," the flash units were placed outside the Industrial Acoustic chamber and were focused through its window with the Fresnel lenses onto a 30.5 × 25 cm translucent sheet attached to the animal chamber superstructure. The flash units were operated by a Grason-Stadler interval timer (Model 471-1) and other appropriate relay and electronic interfacing.

For calibration of the visual stimuli, it was assumed that the effective visual stimulus is the surface intensity of the subject's side of the translucent sheet attached to the chamber. For the visual flash stimuli employed in this research, direct luminance measurement was impossible due to the extremely short duration of the flash (.3 msec). Therefore, the luminance-time integral of the translucent sheet for flash stimuli was determined by comparing photographic prints of the flash stimulus to various exposure length photographic prints of the translucent sheet with a known surface intensity. This known surface intensity was determined by measuring directly, using a Salford Electrical Instruments exposure photometer, the luminance of the translucent sheet illuminated by a Bell and Howell 500 projector. Although the procedure described here is known to be fairly accurate, it should be noted that intensities derived in this manner must be considered as only best estimates due to film reciprocity failure and due to spectral composition differences between the two light sources.

Procedure. The acoustic stimulus (A) was a 117-db. pulse of white noise of 20-msec duration and 5-msec rise–decay time. The visual stimulus (V) consisted of a 3×10^{-2} cd·sec/cm^2 flash, .3 msec in duration with rise time less than .1 msec.

Test stimuli were presented in pairs with an interpulse interval (IPI) of 1 sec. (In this and all subsequent studies reported here the IPI was measured from onset of the first stimulus to onset of the second stimulus in a given pair.) There were four pair configurations: A-A, A-V, V-V, V-A. One pair was delivered every 60 sec. After a 5 min. adaptation period, all animals received, in random order, five presentations of each of the four stimulus configurations on each of 3 days. Testing was conducted with no background sound or illumination in the test chamber.

Results

The mean startle response amplitude in volts (averaged across sessions and subjects) to each component of the several stimulus configurations was 7.50, 2.30 (A-A); 8.10, 0 (A-V); 0, 0 (V-V); and 0, 7.70 (V-A). It is clear from these data that, while the rats exhibited startle reactions to the acoustic signals, they exhibited no overt startle to the visual signals. Moreover, while the acoustic signals produced inhibitory effects, no such effects were engendered by the visual signals. These conclusions are supported by a repeated-measures analysis of variance, $F(7, 28) = 16.75$, $p < .01$. Subsequent Newman-Keuls tests revealed that (a) the response to the second stimulus in the A-A configuration was significantly reduced ($p < .01$) relative to the first, and (b) the response to the A-stimulus in the V-A configuration was not significantly different from that to the first stimulus in the A-A or A-V configurations ($p > .05$ in both cases).

Discussion

The rats in this experiment seemed totally unresponsive to light. They startled only to the acoustic signals and only acoustic signals inhibited subsequent startle reactions. Ison and Hammond (1971) made note of the fact that their rats showed no overt response to the 20 ftc. (215.2 lx) output of a 6-W 125-V bulb. We thought it possible that the more intense light stimulus used here might be sufficient to elicit startle. However, this was not the case. The failure of the light pulse to inhibit a subsequent startle response was consistent with Buckland et al.'s (1969) failure to produce inhibition with a lead time of 640 msec. We were aware of this when we designed the present study, but we elected to employ a 1-sec interpulse interval, nonetheless, because it would enable us to differentiate between responses to the first stimulus and those to the second and because the visual stimulus to be used here (the output of a photo flash unit) was much more intense than the 6-W bulb used by Ison and his colleagues. It had seemed possible that with a sufficiently intense visual stimulus the inhibitory effects might extend to at least 1 sec. Again, this was not the case.

EXPERIMENT 2

This experiment was designed to investigate the effects of visual stimuli at IPIs less than 1 sec in order to document the temporal function for visual inhibition in the rat. By collecting these data we could insure that our previous failure to observe inhibition could be attributed to the long (1 sec) IPI used in Experiment 1 and not to the fact that our rats might have been unable to see the visual stimulus.

Method

Subjects and apparatus. Subjects, apparatus, and the acoustic and visual stimuli were identical to those used in Experiment 1.

Procedure. A visual stimulus and then an acoustic stimulus were delivered to the animals either 4, 16, 64, 250, or 1,000 msec apart. The amplitude of startle to the acoustic stimulus was measured and compared with a control condition consisting of an acoustic stimulus presented alone. The intertrial interval (ITI) was 60 sec. Each animal received five random presentations of the six stimulus configurations every day for 3 days in a silent, dark test chamber.

Results

Figure 1 presents the peak-to-peak response amplitude in volts averaged over sessions and animals, at each of the IPIs. As IPI increases, startle response amplitude decreases up to 64 msec and then begins to return to control level. A repeated-measures analysis of variance showed the observed trend to be statistically reliable, $F(5, 20) = 8.32$, $p < .01$. The Newman-Keuls test showed significant ($p < .01$) differences between the points at 64 and 250 msec and all of the other points.

Discussion

The results of Experiment 2 confirm Ison's finding that visual prestimulation will inhibit the rat's startle response to an intense acoustic signal. Moreover, since in Experiment 1 there was no inhibition at 1-sec IPI, it is clear that our failure to obtain between-mode inhibition in that experiment was due to the long IPI. Thus it appears that relative to visual prestimulation, acoustic prestimulation is effective in producing inhibition over a longer time period. However, for IPIs less than 1 sec the time course for inhibition as seen in Figure 1 is essentially the same as that for inhibition by various acoustic events studied by Stitt et al. (1973) and Stitt et al. (1974). In particular, maximal inhibition is obtained in the same range of IPIs for both visual and acoustic stimuli (64–250 msec).

These data in conjunction with those of Experiment 1 reemphasize the fact that there need be no overt reaction to the first stimulus for it to inhibit responding to the second (Hammond, McAdam, & Ison, 1972; Hoffman & Wible, 1970).

EXPERIMENT 3

As noted previously, Hoffman and Searle (1968) first documented the response latency reducing effect of weak acoustic stimuli presented less than 10 msec before a startle stimulus. This effect was maximal at approximately 5-msec IPI. Subsequent research by Stitt et al. (1973) showed that the offset of an antecedent acoustic signal produced response inhibition (when the lead time was 64 msec) as well as latency reduction (when the lead time was 4 msec). Further, Stitt et al. (1974) found change in the frequency characteristics of an otherwise steady band of noise to engender these startle response modifications at the ap-

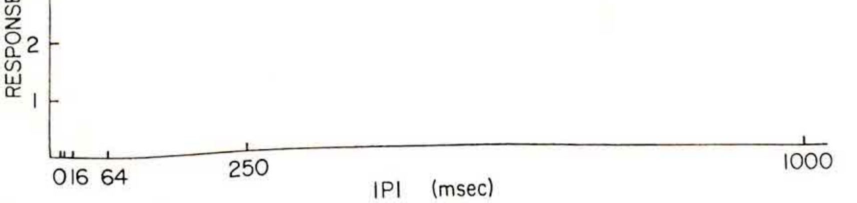

FIGURE 1. Mean peak-to-peak amplitude (across sessions and animals) of acoustic startle in rats when a visual stimulus and then an acoustic stimulus were presented at each of the tested interpulse intervals (IPI). (The dashed line represents the control [acoustic stimulus alone] level.)

propriate interval for each. The purposes of Experiment 3 were (a) to determine whether light stimulation (which had previously produced inhibition) occurring less than 10 msec before an intense sound burst would also reduce startle response latency and if so (b) to obtain the temporal function for this effect.

Method

Subjects. Five experimentally naive male albino rats obtained from Charles River & Co. were used. They were approximately 90–110 days old at the time of testing.

Apparatus. The apparatus for this experiment was identical to that used in Experiments 1 and 2 with one addition to the response detection system. The filtered, amplified output of the stabilimeter coil was displayed in two ways. As in the first two experiments it was passed to one channel of a Tektronix storage oscilloscope (Type 564). In addition, a simple circuit integrated the amplified output by rectifying the current and permitting it to charge a capacitor for 100 msec after the startle stimulus onset. The voltage of the capacitor was then displayed on a high-impedance digital voltmeter with storage capability.

Procedure. A visual stimulus and then an acoustic stimulus were delivered to the animals either 1, 2, 4, 8, or 64 msec apart. The control condition consisted of an acoustic stimulus alone. The 64-msec interval was included in this study as a check that the inhibitory effect previously documented would be replicable in this new group of animals. Each animal received six random presentations (ITI 60 sec) of the six stimulus conditions every day for 3 days in a silent, dark test chamber.

Results

When recorded on the storage oscilloscope, each response yielded an initial downward deflection of the beam, followed by a larger upward deflection and a rapid sequence of damped oscillations. For the purpose of this study, only the initial downward deflection was considered. Response latency was always measured from the onset of the startle-eliciting stimulus to the point at which the initial deflection began. In order to eliminate possible bias, a blind recording procedure was employed. One investigator was responsible for the ordering of the stimuli and the recording of response amplitudes from the digital voltmeter while a second investigator recorded latencies from the oscilloscope. With this arrangement the investigator recording latencies had no way to determine the nature of the stimulus configuration employed on a given trial.

Figure 2 shows response latency in milliseconds averaged across sessions and animals, for each of the IPIs. It is clear from this figure that the light presentations at less than 10-msec IPI do shorten response latency and that light pulses presented at 64 msec before the acoustic startle stimulus do not. A repeated-measures analysis of variance showed this trend to be statistically significant, $F(5, 20) = 76.92$, $p < .01$. The Newman-Keuls test indicated that all the points were significantly ($p < .01$) different from the no-prepulse control level. Comparison of the 2-, 4-, and 8-msec conditions revealed no significant differences between them; however, these points differed reliably from the 1-msec and 64-msec conditions which were also different from each other ($p < .01$ in each case).

Mean startle response amplitude (in volts) for each of the IPIs (averaged across sessions and animals) was 10.26 (no prepulse), 9.39 (1 msec), 9.87 (2 msec), 9.86 (4 msec), 10.78 (8 msec), and 4.80 (64 msec). While a repeated-measures analysis of variance revealed a statistically reliable overall trend, $F(5, 20) = 13.29$, $p < .01$, a Newman-Keuls analysis indicated that only the 64-msec condition was significantly different from any of the other five configurations. This is consistent with previous findings that when using an intense startle-evoking signal, prestimuli that reduce response latency do not necessarily affect response amplitude (Marsh et al., 1975; Stitt et al., 1974).

Discussion

Experiment 1 of the present series revealed that the intense visual stimulus presented to the rats did not elicit overt startle. Experiments 2 and 3 show that, as with acoustic stimuli too weak to evoke overt startle, a flash of light can reduce startle amplitude and shorten response latency depending on the temporal intervals involved. The results of Experiments 2 and 3 also

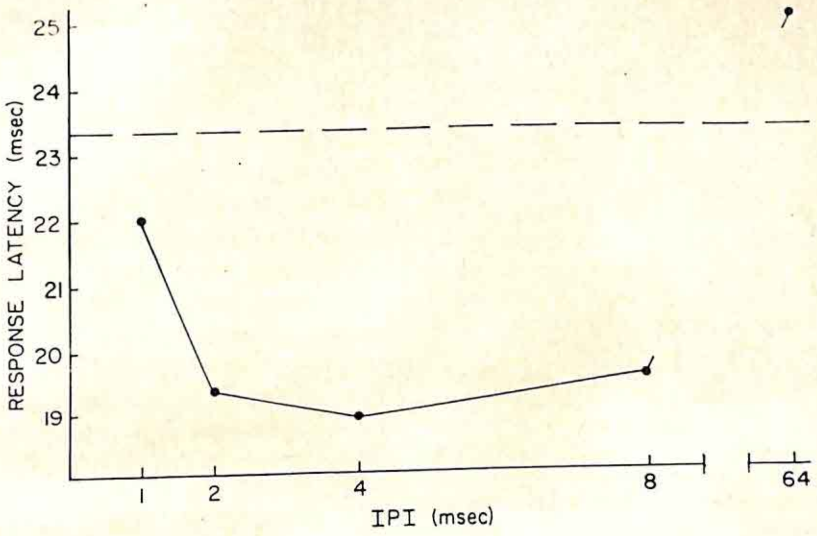

FIGURE 2. Mean startle latency (across sessions and animals) of the rat's response at each of the interpulse intervals (IPIs) tested. (The dashed line represents response latency in the control [acoustic stimulus only] condition.)

reveal other similarities in the effects of acoustic and visual stimuli. As previously noted, the time function for inhibition by visual stimuli is very similar to that of acoustic events. Inhibition is maximal in the region 64–250 msec IPI for the visual stimulus, as well as for noise onset, offset, frequency change, and intensity change (Hoffman & Searle, 1965; Hoffman & Wible, 1970; Stitt et al., 1973; Stitt et al., 1974). In addition, when these visual and acoustic events shorten response latency, their effect is maximal at a point 2–8 msec before the onset of the startle stimulus. These similarities point to the possibility that the light-induced startle modifications are accomplished through the same neural mechanisms that mediate inhibition and latency shift by acoustic stimuli. Whether this is the case or not however, these data clearly imply that the mechanisms responsible for acoustic startle are integrated into the general sensory system rather than restricted solely to the auditory domain.

EXPERIMENT 4

The next step in this investigation was to examine the range of light intensities that produce inhibition and latency reduction of the rat's acoustic startle response. Such data are relevant to an issue raised by Marsh et al. (1975) as to whether both reflex inhibition and latency shift are mediated by the same neural mechanism. In studying the effect of very small shifts in intensity and frequency of an otherwise steady, ongoing noise, Marsh et al. (1975) found differing thresholds for inhibition and latency shift. While frequency changes as small at 1/16 octave produced both effects and intensity changes of as little as .75 db. reduced response amplitude, even changes of as much as 12 db. were ineffective in shortening response latency.

The purpose of Experiments 4, 5, and 6 in the present series was to determine if similar effects obtain in the domain of vision. In particular, these experiments were designed to permit comparison of the thresholds for inhibition and latency shift when the eliciting stimulus for these effects was a flash of light rather than a sound. The goal of Experiment 4 was to determine the weakest light intensity that would engender response inhibition.

Method

Subjects. The same rats used in Experiment 3 served as subjects for this experiment.

Apparatus. The apparatus was basically the same as that used in Experiment 3. One modification was made so that Lafayette 12100 neutral density filters could be mounted in front of the flash unit to vary light intensity.

Procedure. The visual prepulse and the startle stimulus were presented at the optimal inhibitory interval found in Experiment 2 (64 msec). The intensity of the visual prepulse was either 3×10^{-5}, 1.5×10^{-5}, 3×10^{-6}, 3×10^{-7} or 3×10^{-8} cd·sec/cm². While the 3×10^{-5} cd·sec/cm² stimulus is equivalent to only .1% of the visual stimulus intensity employed in Experiments 1–3, it was chosen as the brightest stimulus here because preliminary investigation had revealed that all intensities greater than this level produced reliable and profound inhibition. The other four intensities represented 50%, 10%, 1%, and .1% of 3×10^{-5} (100%).

The amplitude of the response to the acoustic signal after presentation of a visual prepulse was measured and compared to a control condition of the acoustic stimulus only. Each animal received six random presentations (ITI 60 sec) of the six conditions every day for 3 days. There was no background noise or light in the test chamber.

Results

Figure 3 shows response amplitude as a function of light intensity. The amount of inhibition increases with increasing brightness of the prepulse up to the 50% condition where the inhibitory effect appears to be maximal. A repeated-measures analysis of variance showed the trend to be statistically reliable, $F(5, 20) = 44.11$, $p < .01$. The Newman-Keuls test indicated that all the conditions except .1% and 1% differed significantly from the no-prepulse condition. Thus there was significant inhibition with the brightest three levels of stimuli. Within these conditions the 10% condition was different from 50% and 100% (which did not differ from each other, $p < .01$).

Discussion

The brightest stimulus used in this study (.1% of the full flash intensity) appeared to our eyes as a fairly dim stimulus. Thus the inhibitory mechanism appeared to be sensitive to what must be near-threshold light stimuli—as is the case with acoustic stimuli. Moreover, the amount of inhibition

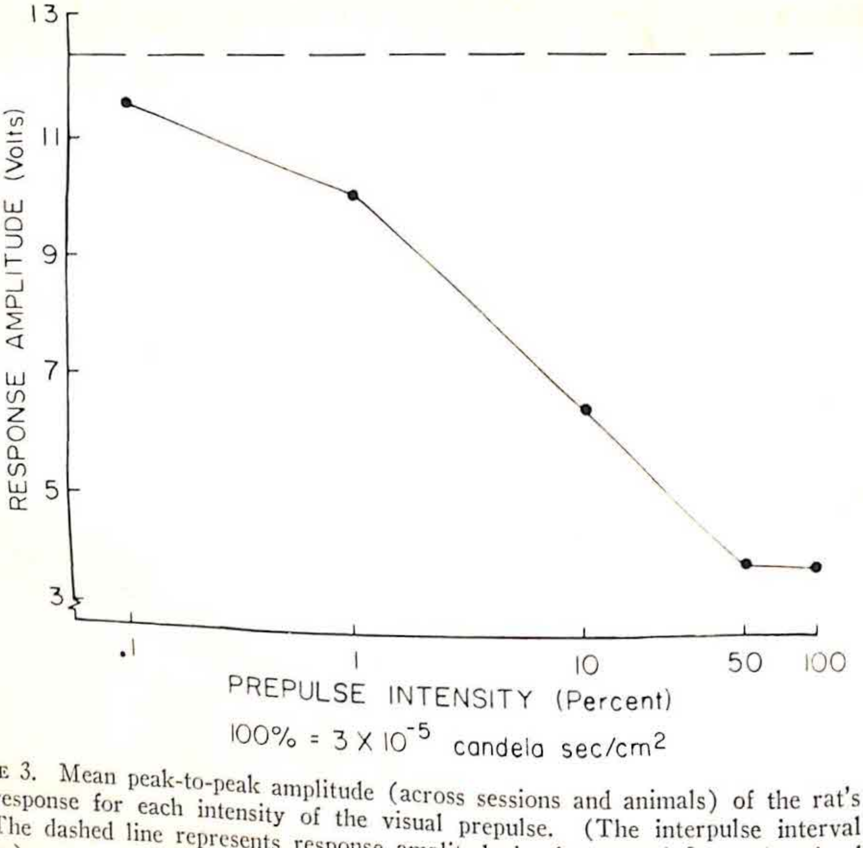

FIGURE 3. Mean peak-to-peak amplitude (across sessions and animals) of the rat's acoustic startle response for each intensity of the visual prepulse. (The interpulse interval was 64 msec. The dashed line represents response amplitude in the control [acoustic stimulus only] condition.)

engendered by the 100% and 50% stimuli was identical to that found for brighter stimuli in preliminary work. This indicates that the maximal effect is obtained quickly along the scale of intensities—a characteristic again directly comparable to those of sound-induced inhibition (Hoffman & Wible, 1970).

EXPERIMENT 5

The goal of this study was to investigate the range of light intensities which will produce startle response latency reduction in the rat. The second question of interest was how the function obtained here would compare to the light-intensity function for inhibition in the previous experiment. To this point the data show the effect of light prepulses to be analogous to that of changes in the antecedent acoustic environment. If this is taken to imply that the two sensory modalities are affecting the same basic mechanisms, then it would be expected that the difference between the intensity functions for inhibition and latency shift found with acoustic stimuli (Marsh et al., 1975) should obtain with visual prestimulation as well.

Method

Subject and apparatus. Subjects, apparatus, and the levels of light intensities were identical to those in Experiment 4.

Procedure. The procedure for this experiment was the same as that in Experiment 4 except that the IPI used was 4 msec—the optimal latency reduction interval obtained in Experiment 3.

Results

Mean response latencies (averaged across sessions and animals) were computed for each stimulus configuration (light intensity in percent of 3×10^{-5} cd·sec/cm^2). For each configuration the mean latency (in milliseconds) was 17.6 (no prepulse), 17.4 (.1%), 17.4 (1%), 17.6 (10%), 17.1 (50%) and 17.4 (100%). It is clear that none of these intensities produced any appreciable latency reduction. Moreover, there was also no effect on response amplitude, and those data are not included here. The particular stimuli used here were apparently too weak to produce the shift. The hypothesis that the rats may not have sensed the visual stimuli can be ruled out since these identical stimuli produced inhibition in Experiment 4 in the same group of rats.

EXPERIMENT 6

This experiment focused on the effect of more intense visual stimuli on startle latency and was begun 5 days after the termination of Experiment 5. Clearly, if the threshold for latency shift was to be specified, it would be necessary to extend the range of stimulus intensities studied.

Method

Subjects and apparatus. Both were identical to those in Experiment 5.

Procedure. The procedure followed here was essentially the same as in the previous experiment. The only change made was in the visual stimuli. The weakest intensity used here was the brightest stimulus in the last experiment (3×10^{-5} cd·sec/cm^2). The most intense stimulus delivered in this study was the full flash intensity used in Experiments 1–3 (3×10^{-2} cd·sec/cm^2) and will be referred to here as 100%. The other stimulus intensities employed were 1.5×10^{-2} (50%), 3×10^{-3} (10%), (3×10^{-4} (1%) and 3×10^{-5} (.1%) cd·sec/cm.^{2}

Results

Figure 4 shows response latency in milliseconds as a function of light prepulse intensity. Latency decreases with increasing intensity of the visual stimulus. A repeated-measures analysis of variance showed a highly significant trend, $F(5, 20) = 111.40$, $p < .01$. A Newman-Keuls comparison indicated that the four brightest stimuli were significantly different from the .1% and the no-prepulse conditions. This test also showed 1% and 10% each to be different from all the other points. The 100% and 50% conditions were different from the four other stimulus levels but not from each other ($p < .01$).

Mean response amplitude (in volts) averaged across sessions and subjects at each light-intensity level (in percent of 3×10^{-2} cd·/cm^2) was 12.21 (no prepulse), 12.05 (.1%), 12.54 (1%), 12.57 (10%), 12.96 (50%), and 14.02 (100%). No significant differences were found between the means of

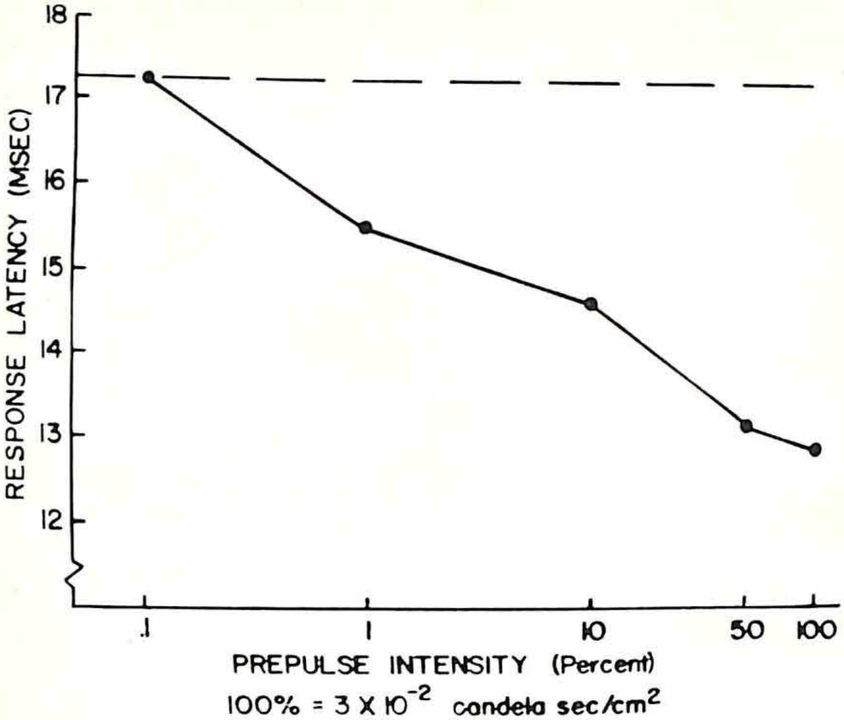

FIGURE 4. Mean response latency (across sessions and animals) of the rat's startle response to the acoustic stimulus for each intensity level of the visual prepulse. (The interpulse interval was 4 msec. The dashed line represents response latency in the control [acoustic stimulus only] condition.)

these conditions in a repeated-measures analysis of variance, $F(5, 20) = 1.24$, $p > .20$. Clearly, at the 4-msec IPI, these prepulse intensities do not affect response amplitude.

Discussion of Experiments 4–6

The data from Experiments 4, 5, and 6 reveal that the threshold for inhibition is much lower than that for latency shift; that is, the latency shift mechanism requires more intense visual stimuli for its activation than does the inhibitory system. This difference is in the same direction as that found by Marsh et al. (1975) with respect to events in the acoustic environment.

CONCLUSIONS

The results of Experiments 1, 2, and 3 make it apparent that, given appropriate temporal parameters, visual prestimulation can produce either response inhibition or latency reduction of the rat's startle reaction to an intense acoustic stimulus. Moreover, the temporal parameters for these effects are very similar to those previously reported for the effects of acoustic prestimulation by Hoffman and his colleagues. On this basis it is concluded that a prestimulus modifies the rat's acoustic startle reaction in essentially the same way, regardless of whether the prestimulus is in the auditory or the visual modality.

The data from Experiments 4, 5, and 6 reveal that the threshold for inhibition by visual prestimuli is lower than that for latency reduction. This finding augments those of Marsh et al. (1975) who found the inhibitory effect to be much more sensitive than latency reduction to acoustical intensity shifts; the present data together with those of Marsh et al . (1975) suggest that latency reduction and response inhibition are mediated by different mechanisms but that both mechanisms can be engaged by either visual or acoustic prestimulation.

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The Context Effect: The Relationship Between Stimulus Preexposure and Environmental Preexposure Determines Subsequent Learning

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Perceptual learning studies indicate that stimulus preexposure enhances subsequent learning, while latent inhibition studies indicate that stimulus preexposure retards subsequent learning. An analysis of the two paradigms suggests that predictions in regard to the effects of stimulus preexposure must take into account not only the novelty of the stimulus, but the relationship of that novelty to that of the environment at the time of testing. Two studies with identical designs, one with children and one with rats, are reported. In both studies, enhancement of learning is achieved when a new stimulus is presented in an old environment or an old stimulus in a new environment as compared to either a new stimulus in a new environment or an old stimulus in an old environment. This demonstrates latent inhibition and perceptual learning, and shows that the direction of the effects of stimulus preexposure is dependent on the relative novelty of the environment. A two-process theory based on enhancing salience through conditioning and arousal is proposed to account for the pattern of results.

The effect of stimulus preexposure on subsequent learning has received considerable attention over the last two decades. Within this extensive literature there are two distinct paradigms which produce apparently contradictory results. On the one hand, there are the perceptual learning studies which demonstrate that stimulus preexposure results in facilitation of later learning relevant to that stimulus (e.g., Gibson & Walk, 1956; Oswalt, 1972; see reviews by Epstein, 1967, and Gibson, 1969). On the other hand, there are the latent inhibition studies which demonstrate that prior exposure to the to-be-learned stimulus results in a decrement of later learning relevant to the stimulus (e.g., Lubow & Moore, 1959; Reiss & Wagner, 1972; Rescorla, 1971; see reviews b Lubow, 1973a, 1973b).

An analysis of the two paradigms suggests important differences in the operation relating preexposure to testing. In th perceptual learning paradigm, experimenta and control groups are reared in their hom environments, and the experimental grou is exposed to the to-be-tested stimuli i that environment. Testing occurs in a ne environment, typically a Grice discrimination apparatus (Grice & Salz, 1950). Thu the experimental group is tested with familiar stimulus in a new environment an is compared to a control group which tested with a new stimulus in a new environment. The paradigm is denoted, in term of the conditions *during testing*, in Table 1 For such a comparison, as in the perceptu learning procedure, S_oE_n results in bette learning than S_nE_n (e.g., Gibson & Walk 1956).

However, in the latent inhibition paradigm, both the experimental and the control groups are preexposed to the apparatu in which later testing will occur. It is durin this period of environmental preexposur that the subjects in the experimental grou

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TABLE 1

Relationship Between Stimulus and Environmental Novelty at Time of Testing

Paradigm	Experimental group	Control group
Perceptual learning	S_oE_n	S_nE_n
Latent inhibition	S_oE_o	S_nE_o

Note. S = stimulus, E = environment, n = new, o = old.

are also preexposed to the critical stimulus. The relationships between stimulus and environment at the time of testing are described in Table 1. During testing, the stimulus-preexposed group is confronted with an old stimulus in an old environment, while the nonpreexposed stimulus group faces a new stimulus in an old environment. With such conditions, learning proves to be slower for the S_oE_o group compared with the S_nE_o group (e.g. Reiss & Wagner, 1972).

In both the perceptual learning paradigm and the latent inhibition paradigm it would appear that a condition of *contrasting* novelty or familiarity of the environment with that of the stimulus provides a better condition for learning than the absence of such a contrast. The apparent conflict in results from the two paradigms, then, is a result of comparisons on the basis of stimulus preexposure rather than on the basis of this contrast. In perceptual learning the stimulus-preexposed group is the contrast group (S_oE_n) during testing, and in the latent inhibition paradigm it is the nonpreexposed group that is the contrast group (S_nE_o) during testing.

It is quite clear that there are many other differences between the two paradigms. Therefore it was decided to combine, in one experimental design, the basic elements from both the perceptual learning paradigm and the latent inhibition paradigm. If the preceding conjectures are correct, one would expect to be able to produce either a facilitatory or an inhibitory effect of stimulus preexposure, depending on whether the stimulus-preexposed groups are tested in a new environment or in the environment in which they were preexposed.

The predictions in regard to the four groups generated by the analysis are $S_oE_n > S_nE_n$ (perceptual learning); $S_nE_o > S_oE_o$ (latent inhibition). Inasmuch as it is predicted that contrast generally is more efficacious than no contrast, even in conditions other than that of the perceptual learning and latent inhibition paradigms, two new paradigms with appropriate predictions can be generated: $S_oE_n > S_oE_o$; $S_nE_o > S_nE_n$. Predictions are not made in regard to the relative efficacy of S_nE_o vs. S_oE_n, nor S_nE_n vs. S_oE_o as it is likely that these will be experiment-determined, depending on specific properties of the stimuli and the environment.

The preceding predictions were tested in two different experiments. Experiment 1 used children in a discrimination learning task, while Experiment 2 used rats in a discrimination learning task. The experimental designs for the two studies were identical.

Experiment 1

Method

Subjects. Eighty boys and 80 girls, attending five kindergartens in Rishon LeZion, Israel, served as subjects. The mean age was 5 years. Subjects were randomly assigned to groups, but with equal number of boys and girls in each group.

Apparatus. The apparatus consisted of two different environments and two different sets of stimuli. The environment was defined as including all of the stable cues during either the prexposure period or the testing period. The major sources of these cues were the walls of the experimental cubicle and the table on which preexposure and testing were conducted. Two different environments were constructed. Environment R was constructed from two plywood sheets painted bright red and hinged on the longer axis. Placed in a corner of the kindergarten room, they made a small test cubicle (120 × 120 × 180 cm). Within the cubicle were an irregularly shaped red table, a small red chair for the subject, and a larger red chair for the experimenter. The table was approximately 80 × 60 cm. The irregular shape was achieved by acute and obtuse angles. Environment B was similar to Environment R except that everything was painted blue, and the irregular shape of the table was composed of curves rather than angles.

An opaque screen (in the same color as the environment) which could be raised and lowered was attached to the front of each table, separating subject from experimenter. Four wells and adjacent microswitches were embedded in the table top. A stimulus form was placed on top of each well. An electric timer was activated when the opaque screen was raised, and was stopped via the microswitch when a stimulus form was lifted. A marble was placed in the well under the correct stimulus form. Along the edge of the table there was a series of 20 holes in which the subject could accumulate the marbles earned for correct responses. Two sets of stimuli were employed. Stimulus Set C consisted of four objects, each of which was irregularly shaped but basically circular in form. The objects were made from plaster of paris and were approximately 8 × 8 × 4 cm. Each object in the set was painted with three of the four colors, blue, silver, orange, and dark green with white spots. Stimulus Set S also consisted of four objects made of the same material as the objects of Stimulus Set C . Each object was irregularly shaped but basically square in form. The sizes were approximately the same as that for Set C. Each object was painted with three of the four colors, light green, brown, yellow, and light orange.

Procedure. The 160 subjects were randomly divided into four preexposure groups. These four groups were preexposed to the different possible combinations of the two stimulus sets (C or S) and the two environments (R or B). The resulting four groups can be designated CR, SR, CB, and SB, where the first letter indicates the stimulus set, and the second letter indicates the environment which was preexposed.

Each of the four preexposure groups was subsequently subdivided into four test groups to secure one of the four combinations of environmental (R or B) and stimulus conditions (C or S) during testing. Thus the complete experimental design consisted of a 4 × 4 factorial arrangement of the four preexposure conditions and the four test conditions. Table 2 describes the 4 × 4 design and the manner in which the 16 groups can be identified to form four major comparison groups: Group S_oE_o where the stimulus and environment remain the same from preexposure to test; Group S_oE_n where the stimulus remains the same but the environment changes; Group S_nE_o where the stimulus changes but the environment remains the same; and Group S_nE_n where both the stimulus and the environment change from the preexposure to the test period.

TABLE 2

RELATIONSHIP BETWEEN PREEXPOSURE AND TEST CONDITIONS IN TERMS OF CONTEXT DURING TESTING FOR EXPERIMENTS 1 AND 2

Group		Condition			
Experiment 1	Experiment 2				
CR	LC	S_oE_o	S_nE_o	S_oE_n	S_nE_n
SR	WC	S_nE_o	S_oE_o	S_nE_n	S_oE_n
CB	LR	S_oE_n	S_nE_n	S_oE_o	S_nE_o
SB	WR	S_nE_n	S_oE_n	S_nE_o	S_oE_o

Note. S = stimulus, E = environment, n = new, o = old. In Experiment 1, C = circular, S = square, B = blue, R = red; in Experiment 2, L = lemon, W = wintergreen, C = circular, R = rectangular.

In the preexposure phase, each subject was invited into the cubicle and seated in front of the lowered opaque screen. He or she then received the following instructions:

> "When I raise the screen you will see a table and several objects on the table. Please do not touch them. Sit with your hands crossed or by your side, as you like. I want to measure with this watch how long you can sit quietly, watching the table and the things on it, without saying or doing anything . . ."

After instructions were read, the screen was raised, and the subject allowed to look at the table and objects for 10 min. Object positions on the table were the same across all subjects viewing each stimulus set. At the end of the preexposure period the subject was given a 2-min. rest during which he or she left the cubicle.

To begin the test period, the subject was again invited into the cubicle, and read the following instructions:

> "When I open this screen you will see a table with some things on it. But now I have hidden a marble under one of the objects. As soon as I open the screen please pick up the objects and find out which one has the marble. When you find the marble put it in one of the holes in the side of the table. Hold the object under which the marble was found, and look at it until I finish arranging the things. When you raise an object and don't find a marble, let go of it without trying to put it back in its place. It is very important that you find the marble as quickly as possible; therefore, start looking for it as soon as I raise the screen. If you find it immediately you will receive a high score."

During the instruction period and between trials the screen was lowered. The same object of a given set was the positive stimulus across all trials and across all subjects receiving that set. The position of the objects was completely randomized across trials and across subjects. All subjects received 20 trials, each lasting 30 sec. If the subject found the correct form before 30 sec, he or she had to hold it and look at it until, at the end of the 30-sec period, the experimenter removed it and closed the screen.

The following measures were recorded: (a) *response time*—the time, in seconds, between the raising of the screen and the lifting of the object under which a marble was placed; (b) *number of errors*—the number of objects raised under which there was no marble (since a correction procedure was employed, the subject could make more than 1 error per trial); (c) *number of trials to criterion*—the number of trials to the occurrence of three consecutive trials on which the first object choice was correct.

Results

The results for the three dependent variables for each of the 16 groups are shown in Figure 1. As can be seen, all three measures show the same general trend with a larger number of errors, larger number of trials to criterion, and longer reaction time associated with conditions of no contrast between stimulus and environmental novelty (S_oE_o, S_nE_n) and fewer number of errors, fewer number of trials to criterion, and faster

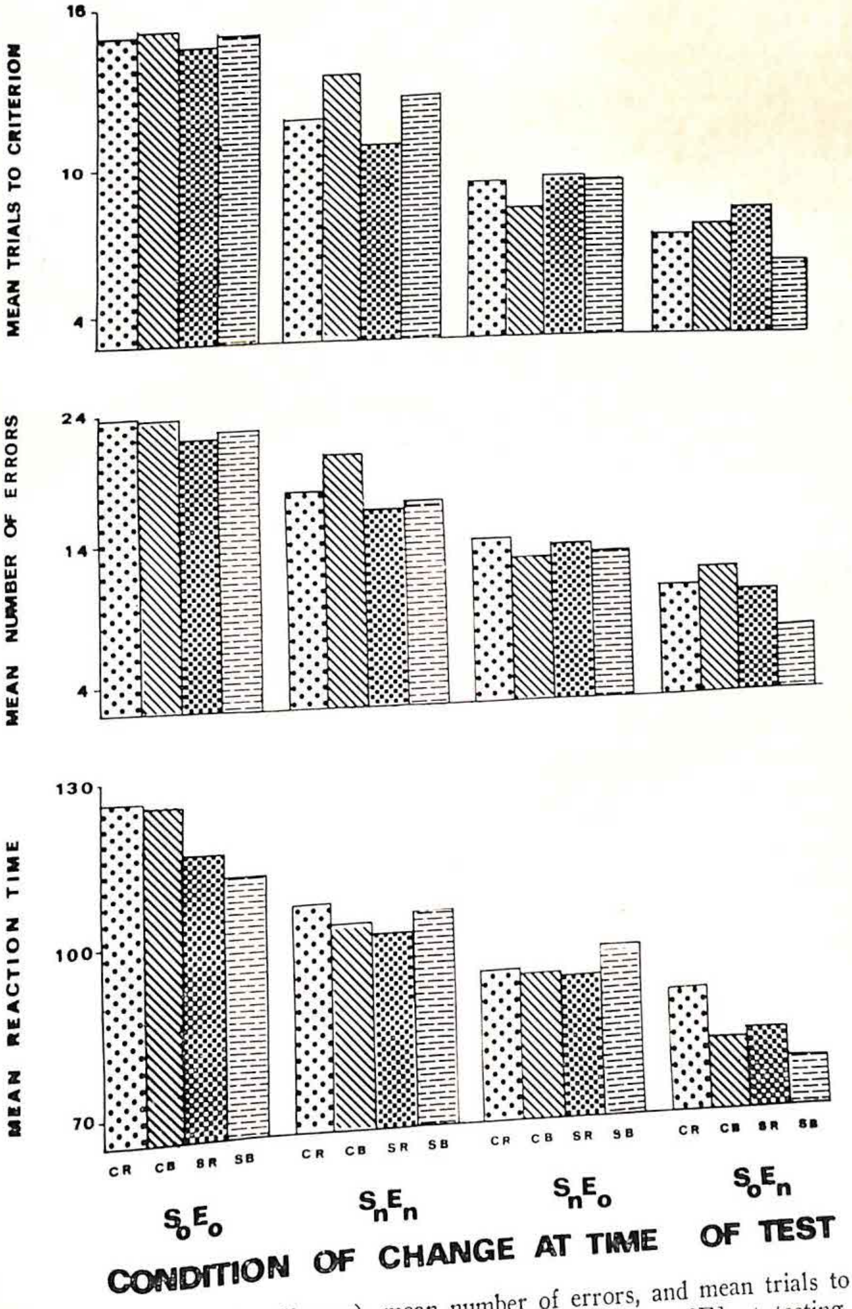

FIGURE 1. Mean reaction time (in sec), mean number of errors, and mean trials to criterion for all subgroups. (Indicated are the stimuli [S] and environments [E] at testing [stimuli: C = circular, S = square; environments: B = blue, R = red] and the relationship of these to the preexposure [o = old, n = new].)

reaction times associated with conditions of contrast between stimulus and environmental novelty (S_nE_o, S_oE_n). Also, there is no overlap between subconditions of the different major groups; that is, the highest and lowest score of subgroups within a major category do not overlap with those of any of the other subgroups from a different major category.

The first analysis of these data was accomplished with a 4 × 4 analysis of variance (ANOVA). The data were grouped so that one of the main factors reflected the four types of change conditions from preexposure to test (i.e., conditions labeled S_oE_o, S_nE_n, S_oE_n, and S_nE_o). The second main factor reflected particular values for stimuli and environments at the time of testing, (i.e., combinations labeled CR, CB, SR, and SB).

All three dependent measures, response time, number of errors, and trials to criterion, exhibited the same pattern. The effects of type of change from preexposure to test were significant, $F(3, 144) = 47.24$, $p < .01$, $F(3, 144) = 76.95$, $p < .001$, and $F(3, 144) = 112.41$ $p < .001$, respectively, while neither the effects of the particular stimulus–environment combination nor the interaction of the latter factor with the first were significant.

Subsequent analyses were therefore performed by means of a 2 × 2 ANOVA where one factor was contrast versus no contrast between the familiarity or novelty of the stimulus and the environment, and the other factor was whether the test stimulus was preexposed or novel.

Again the results for the three dependent measures, number of errors, trials to criterion, and reaction time, are consistent. Contrast versus no contrast was highly significant, $F\text{s}\ (1, 156) = 275.16$, 105.65, and 183.46, respectively, $p\text{s} < .001$, while the effects of stimulus preexposure were not significant, $F(1, 156) = 1.96$, $p > .10$; $F(1, 156) < 1$; $F(1, 156) = 1.52$, $p > .10$, respectively. On the other hand, the Stimulus Preexposure × Contrast interaction was significant on each measure and exactly reflects the predictions, $F\text{s}(1, 156) = 48.03$, 105.65, and 183.46, respectively, $p\text{s} < .001$. These interactions may be observed in Figure 1. Preexposure to a stimulus interferes with subsequent learning when the environment is also preexposed (S_oE_o vs. S_nE_o), but facilitates learning when the environment is novel (S_oE_n vs. S_nE_n). These interpretations of the interactions are supported by an application of the Scheffé test. For each dependent measure, each of the conditions S_oE_o, S_nE_n, S_nE_o, and S_oE_n was significantly different from every other ($p < .001$) with the exception of the reaction time comparison between S_oE_n and S_nE_o, which did not differ reliably.

EXPERIMENT 2

The purpose of Experiment 2 was to determine if the effects described in Experiment 1 were limited to the particular procedures and subjects that were employed or represent a more general pattern. Therefore, the exact same experimental design was used, but with a different set of procedures and with different subjects. The substitution of rats for children was deemed to be of particular importance since most of the perceptual learning and latent inhibition literature described data from rat subjects. However, in spite of the fact that most of the studies employed visual or auditory discriminative stimuli, odorous stimuli were used in the present study, in particular, lemon and wintergreen. Rats have been shown (O'Grady & Jennings, 1972) easily to learn to discriminate between these two odors without display of initial preference. An olfactory discrimination problem was used in order to utilize the dominant sensory modality in the rat, thus making it comparable to using the visual modality in humans in Experiment 1. The implicit assumption is that general behavioral laws are more easily uncovered when cross-species comparisons are made on ecologically comparable systems.

Method

Subjects. Subjects were 80 male Charles River rats 75–110 days of age. Following delivery from the Tel-Aviv University animal laboratory, animals were housed in individual cages under reverse-cycle lighting.

Apparatus. The apparatus consisted of two types of environments: circular and rectangular cages, each constructed of opaque plastic. The circular cages were 11.0 cm high with a diameter of 28.0 cm. The rectangular cages were 29.0 × 20.0 × 13.5 cm. Two transparent plastic cylinders were attached to each cage with the open end of the cylinders facing into the cage. The angle between the two cylinder openings was 90°, measured by the converging lines of the long axes of the cylinders. Several small holes were drilled into the otherwise closed outer end of each cylinder. A 30-ml Pyrex beaker was clipped to the outer surface of the end of each cylinder. A .75-cm hole was drilled on the top of each cylinder, 2.0 cm from the back wall, through which the delivery tube of a mechanical feeder was inserted during testing.

For the circular cages the tunnels were 6.0 cm long with a diameter of 7.0 cm, and the tunnel surface was deeply ridged. For the rectangular cages, the tunnels were 7.5 cm long with a diameter of 7.0 cm, and the smooth surface of the bottom interior was coated with a layer of sand to enhance the contrast between the different environments.

The circular cages were covered with plastic grids, .75 cm deep with 1.5 cm open squares, whereas the rectangular cages were covered with flat metal with several long slotlike openings.

The olfactory stimuli which were to be discriminated were provided by extracts of lemon and wintergreen, manufactured by the Ta'am Verreach and McCormick food companies. Prior to each experimental session, 20 drops of the appropriate liquid were placed on a clean, dyed cotton ball which was then placed in a Pyrex beaker. The "neutral" stimulus consisted of 20 drops of tap water on a clean, dyed cotton ball. The cotton balls were dyed to eliminate the possibility of discrimination on the basis of brightness differences imparted from the odor source.

During the intertrial interval (ITI) of testing, entry to the cylinders was prevented by the insertion into the cage of a single plywood sheet which stood between the rat and the entrances.

Procedure. For the preexposure phase, animals were assigned to one of two environments, circular (C) or rectangular (R), and to one of two olfactory stimuli, lemon (L) or wintergreen (W). For the testing phase they were further subdivided for assignment to either the familiar or novel environment and to either the familiar or novel stimulus. Thus, there were four major groups and 16 subgroups identical to the design of Experiment 1 as illustrated in Table 1.

On Day 1 of the experiment each subject was placed in the preexposure environment where it lived for the duration of this phase, 14 days. The preexposed olfactory stimulus was presented twice daily for a total of 2 hr. each day during this period. The odorant was attached to one of the tubes and the neutral odor source was attached to the other. The position of the two stimuli varied randomly.

Testing took place over a period of 4 days following preexposure. The procedure was identical for all animals. Each subject was removed from its preexposure cage and placed in a holding cage for 5 min. Animals tested with the familiar environment were returned to the preexposure cage and those tested with the novel environment were placed in the type of cage in which they were not preexposed. At this point entry to the cylinders was blocked. Before each test trial, the test odor source (i.e., either familiar or novel) was attached to one of the cylinders, the neutral stimulus was attached to the other cylinder, and the barrier was raised.

Subjects were permitted to make one correct response per trial. A correct response was defined as entry into the cylinder to which the test odor container was attached so that the subject's nose came within 1.0 cm of touching the back end of the tunnel. Following a correct response, subjects were reinforced with a .45-gm Noyes pellet and the trial was terminated. If the animals did not leave the tunnel within 5 sec, he was removed from it. This type of handling was not frequently required. After the rat left the tunnel, the barrier was again inserted. The next trial was delayed for approximately 60 sec, as measured by the time between successive liftings of the barrier.

Partial entry into the odor or nonodor tunnels and complete entry into the nonodor tunnels were not reinforced. If the subject did not make the correct response within 60 sec, the trial was terminated. At the end of each trial, the barrier was replaced and the odor sources were removed. The cylinder to which the odor source was reattached was determined randomly.

There were 20 trials on each test day, separated by the ITI of 60 sec. Thus, there was a total of 80 trials. The number of correct responses and the latency of the correct response were recorded. Two measures of latency were analyzed. The first was measured from trial onset (i.e., raising the barrier) to the correct response and included those trials on which no response was given. On these trials subjects were assigned a score of 60 sec. The second latency source was measured from trial onset to the correct response but did not include those trials on which no correct response was given.

Results

The results for the three dependent variables for each of the 16 groups are shown in Figure 2. In order to be consistent in the display of data for Experiments 1 and 2, the mean number of correct responses is shown in Figure 2 in terms of the mean number of errors. An error is defined as the absence of a correct response during a trial. As can be seen, all three measures show the same general trend with fewer correct responses and longer latencies associated with conditions of no contrast between stimulus and environmental novelty (S_oE_o, S_nE_n) in comparison with a larger number of correct responses and shorter latencies associated with conditions of contrast between stimulus and environmental novelty (S_nE_o, S_oE_n). In addition, there is virtually no overlap between subconditions of the different major groups.

The first analysis was a 4 × 4 ANOVA. The data were grouped so that one of the main factors reflected the four types of change conditions from preexposure to test (i.e., the conditions S_oE_o, S_nE_n, S_oE_n, and

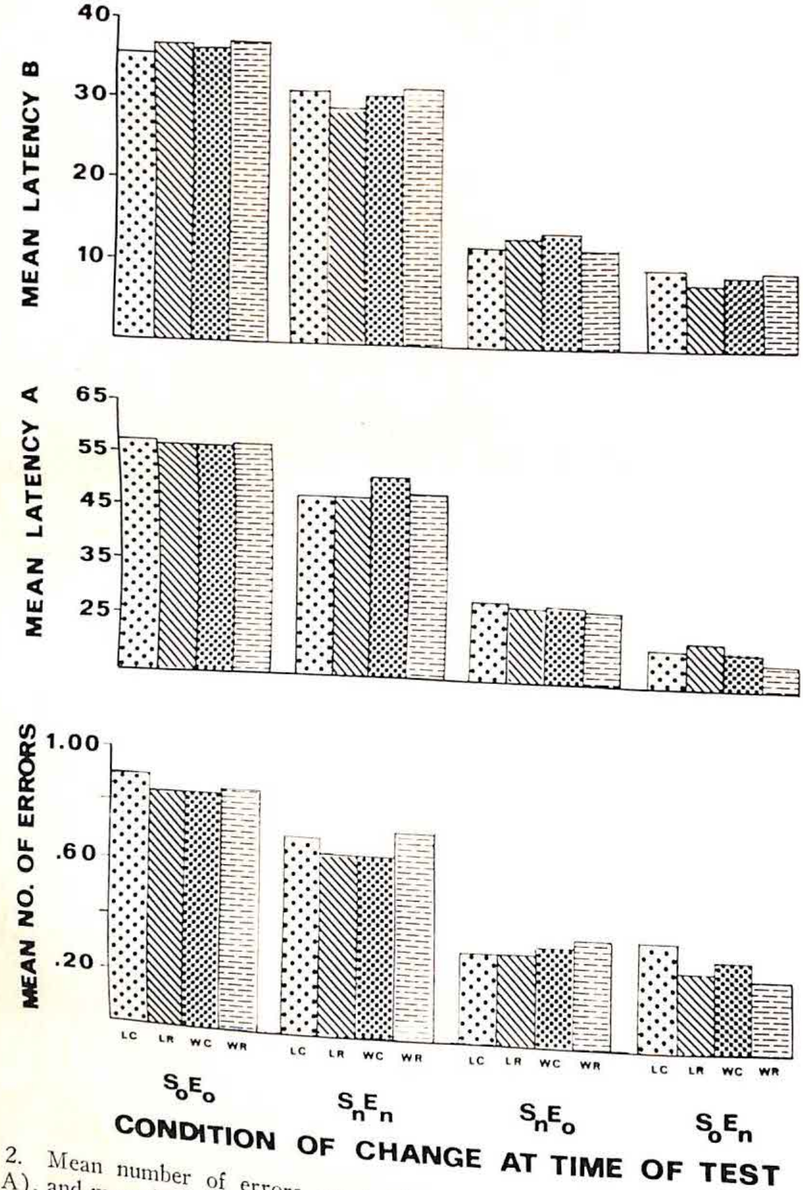

FIGURE 2. Mean number of errors per trial, mean latency (in sec) to correct response (Latency A), and mean latency (in sec) to correct response not including those trials on which no correct response was given (Latency B) for all subgroups. (Indicated are the stimuli [S] and environments [E] at testing [stimuli: L = lemon, W = wintergreen; environments: C = circular, R = rectangular] and the relationship of these to the preexposure [o = old, n = new].)

S_nE_o). The second main factor reflected the particular combination of stimuli and environments at the time of testing (i.e., CL, CW, RL, and RW.

All three dependent measures, number of correct responses and both types of latency, exhibited the same pattern. The effects of type of change from preexposure to test was significant in each case, $Fs(3, 64) = 426.4$, 895.21, and 690.23, respectively, $p < .001$, while neither the effects of the particular stimulus–environment combination nor the interaction of the latter factor with the first was reliable.

All subsequent analyses were therefore performed with the 2×2 format where one factor was contrast versus no contrast between the familiarity or novelty of the stimulus and the environment, and the other factor designated whether the test stimulus was preexposed or not preexposed.

The results for each of the three dependent measures, number of correct responses, latency to correct response, and latency to correct response not including those trials on which no correct response was given, were similar. The contrast versus no contrast effect was highly significant in each, $F(1, 76) = 1{,}281.97$, 2,393.65, and 1,825.59, respectively, $ps < .001$. The effects of stimulus preexposure also were significant, $Fs\ (1, 76) = 4.18$, 4.81, and 10.81, $ps < .05$, $< .05$, and $< .01$ respectively. From Figure 2 it would appear that there was a small overall effect in the direction of making learning to the preexposed stimulus more difficult and slower than to the novel stimulus. Nevertheless, the Stimulus Preexposure × Contrast interaction also was significant, and again, exactly reflects the predictions, $Fs\ (1, 76) = 22.79$, 106.45, and 168.52, $ps < .01$, $< .001$, and $< .001$, respectively. These interactions may be observed in Figure 2. Thus, preexposure to a stimulus interferes with subsequent learning when the environment is also preexposed (S_oE_o vs. S_nE_o), but facilitates learning when the environment is novel (S_oE_n vs. S_nE_n). This interpretation of the interaction was supported by an application of the Scheffé test. For each dependent measure, each of the conditions S_oE_o, S_nE_n, S_nE_o, S_oE_n was significantly different from every other ($p < .001$) with the exception of the comparison between the S_oE_n and S_nE_o conditions, which did not differ reliably for mean number of correct responses.

DISCUSSION

The two experiments clearly demonstrate that the facilitatory effects of stimulus preexposure are dependent on producing a contrast in novelty between the test stimulus and environment which is not afforded in the comparison condition, while the decremental effects of stimulus preexposure are due to the removal of such a contrast otherwise occurring in the comparison condition. Thus, in the same experiment both latent inhibition and perceptual learning were obtained.

Latent inhibition was demonstrated by comparing the groups receiving the same environment during preëxposure and test, but a new stimulus during test, with the group receiving the same environment and the same stimulus during preexposure and test. These are the operations in the latent inhibition paradigm, and as repeatedly found (cf. Lubow, 1973a) this situation results in slower learning for the stimulus-preexposed group as compared to the nonpreexposed group.

Perceptual learning was demonstrated by comparing the group receiving the same stimulus during preexposure and test, but tested in a different environment, to the group receiving a new test stimulus in a new environment. These are the typical operations in the perceptual learning paradigm. Typically, this procedure results in superior learning for the stimulus-preexposed group as compared to the nonpreexposed group. (cf. a summary of the Gibson–Walk studies in Epstein, 1967, or Gibson, 1969).

In summary, then, a major paradox in the literature of learning seems to be resolved. The most effective conditions for learning are those in which there is a contrast between the novelty of the environment and the novelty of the stimulus. Both a new stimulus in an old environment and an old stimulus in a new environment are relatively effective conditions for learning compared to an old stimulus in an old environment or a new stimulus in a new environment.

The fact that the same pattern of results was obtained from children and rats under diverse circumstances indicates that the contrast variable has broad external validity and therefore may be a critical parameter for the construction of a general behavior theory.

What explanatory framework can be constructed to account for the pattern results, $S_oE_n > S_nE_o > S_nE_n > S_oE_o$? It may be assumed that learning is dependent on the elicitation of a perceptual response to the experimentally relevant stimuli which in turn is dependent upon the *salience* of the stimuli. To be more specific, it may be assumed that the consequences of increasing the salience of a stimulus are (a) to make the figure–ground distinction more sharp on the phenomenal level, that is, to make the stimulus stand out from the background; (b) to elicit receptor orienting responses of the gross motor type, described by Pavlov (1927) as the "investigating reflex;" (c) to elicit the pattern of physiological responses described by Sokolov (1963) as the orienting response; and finally (d) to increase the capability of the stimulus to enter into associations with other events, presumably as a result of one or more of the previous attributes.

In regard to the antecedent conditions for changing salience, the major parameters are those of intensity and novelty, and their effects have been well documented (e.g., Berlyne, 1960; Sokolov, 1963). In the simplest case, novelty, which is of concern in this paper, is an experimentally manipulable stimulus parameter which is measured by frequency or duration of stimulus exposure, or by some combination of the two. Although the effects of such preexposures are presumed to reduce salience, the rate of salience reduction as a function of preexposure is modifiable by a number of factors including intensity of the preexposed stimulus (Crowell & Anderson, 1972; Schnur & Lubow, Note 1), time between stimulus preexposures (Lantz, 1973; Schnur & Lubow, Note 1), and the presence of a second stimulus in a conditioning relationship to the target stimulus (Lubow, Alec, & Arzy, 1975). However, the results of the two experiments presented in this paper suggest that *absolute* stimulus novelty is less important in affecting salience than the *relative* novelty of the stimulus to the environment. Indeed it would appear that salience is virtually independent of absolute stimulus novelty. Thus S_oE_n and S_nE_o, both high in relative novelty compared with S_oE_o and S_nE_n, are conditions for facilitating learning, even though in the S_oE_n case there is low absolute stimulus novelty. Experiments which report an effect of novelty on learning, attention, or orienting responses typically do not differentiate between absolute and relative novelty and, accordingly, the two are confounded. It would now appear that the appropriate identification of the independent variable in those studies should be in terms of the relative novelty between the stimulus and environment. It is the relative novelty that is important for producing a perceptual response.

Why, in the S_oE_n condition, is it the S and not the E that benefits from the contrast by eliciting increased salience? In any experiment, there is an inherent distinction between stimulus and environment, so that the stimulus always produces a disproportionate amount of salience for the subject compared to the environmentally generated stimuli. This occurs because the experimenter-generated stimulus is the independent variable which means that it is dynamic, in a discrimination experiment changing in time and place across trials. The environmental stimuli are, by comparison, static, stable in time and place. All of these conditions are conducive to making the stimulus–environment relationship one of figure–ground. The preexposure manipulations produce a change in stimulus salience, but never reduce it below the salience level of the environmentally generated stimuli. This explains why there is any learning at all to S_oE_n.

But why should S_oE_n be superior to S_oE_o? This can be reconciled within the framework of the conditioned attention theory (Lubow, Alek, & Arzy, 1975) or a habituation model (Lantz, 1973). In the S_oE_n situation, E_n may serve as an external inhibitor (or dishabituator) so that the original perceptual response to S is reinstated.

To account for the superiority of S_oE_n to S_nE_o, a general arousal construct may be proposed, whereby new environments independently of S_n or S_o produce conditions conducive to learning. This may also serve to explain the superiority of S_nE_n to S_oE_o. It is plausible that arousal, too, may have its ultimate effect on learning by increasing stimulus salience. Kahneman has summarized some of the relevant research: "This research demonstrates that high arousal causes attention to be concentrated on the dominant aspects of the situation at the expense of other aspects" (1973, p. 38). Perhaps, then, both the conditioning and the arousal mechanisms work in concert to affect stimulus salience.

All of the above theorizing is, admittedly, post hoc. However, it may provide new stimuli on old backgrounds, highlighting some intriguing data.

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Long-Term Retention of Early Pavlovian Fear Conditioning in Infant Rats

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That organisms cannot remember events occurring during infancy may be the result of common forgetting processes exacerbated by the organism's increase in size during development or a unique process such as insufficient neurological development at the time of the early experience. To establish the uniqueness of infantile forgetting, size change was made irrelevant by exposing infant rats to "off-baseline" Pavlovian fear conditioning and assessing the effect of an apparatus-free conditioned stimulus upon independently established bar pressing. In Experiment 1, bar pressing by rats exposed to Pavlovian contingencies when 20–22 days old was substantially suppressed by the conditioned stimulus both 1 and 42 days after conditioning. In Experiment 2, pups conditioned when 17–19 and 20–22 days old again showed excellent retention, whereas pups conditioned when 11–13 and 14–16 days old showed total forgetting 42 days later. In Experiment 3, pups conditioned when 14–16 days old remembered well after 5 days, less well 10 days later, and not at all after 20 days. These findings suggest that size change may contribute to the forgetting of events occurring late in development, but that neurological immaturity may underly the forgetting of earlier events.

It seems well established that events occurring during infancy are much more likely to be forgotten than events occurring during adulthood (for a recent review, see Campbell & Coulter, in press); however it is not clear why this is so. In several papers Campbell and others (Campbell, Riccio, & Rohrbaugh, 1971; Campbell & Spear, 1972; Campbell & Coulter, in press) have suggested two alternative explanations for infantile forgetting, one "neurological" and the other "psychological." In the first case the absence of memory for early experiences is attributed to the absence early in life of brain structures, pathways, or substances that are necessary for the mediation of long-term memory (LTM); in the second case the forgetting of early experiences is attributed to forgetting mechanisms observable at all stages of development but which are exacerbated by environmental changes induced by growth. The neurological account is of particular interest. It suggests that while memory failures for early events may not be due to processes studied frequently in adults, they may be useful for determining the physiological concomitants of LTM in general (just as are memory failures shown by lesioned adults).

Despite increasing interest in the possibility that neurological immaturity is a major determinant of infantile forgetting (e.g., Campbell, Misanin, White, & Lytle, 1974), the results of nearly all developmental memory studies to date do not require this kind of explanation. Most of the data derive from traditional learning tasks (i.e., spatial avoidance, active avoidance, escape learning, bar pressing) where the demonstration of retention depends largely upon the recognition of spatial apparatus cues and the performance of highly practiced

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responses. To an organism that has grown significantly during the retention interval, not only will the apparatus appear different (Perkins, 1965) but the originally learned response may prove to be grossly inappropriate. Thus, an organism that learned a task as an infant may perform poorly after it has grown, not because it lacked certain central structures while young but because both the cues and response requirements have been drastically changed by the organism's change in size.

If one is ever to seriously test a neurological account of infantile forgetting, it would seem essential to eliminate or to substantially reduce the potential effects of size change upon performance during the retention test. One possible way of reducing these effects is to compensate for the increased size of the subject by increasing the size of the apparatus. Unfortunately, investigators who have used this procedure (Fiegley & Spear, 1970; Thompson, Koenigsberg, & Tennison, 1965) have found that its major effect is to add a new source of performance disruption to the test. Since disruption produced by explicit changes in the test situation is generally regarded as evidence of *good* retention (e.g., McAllister & McAllister, 1965; Perkins & Weyant, 1958; Saltz & Asdourian, 1963), it becomes difficult to interpret the significance of these results. In fact, one could argue that if increased size does significantly change the organism's perception of the original learning situation, infantile forgetting could just as well be interpreted as evidence of excellent retention by organisms trained when they were not fully grown.

Rather than trying to compensate for changes in size, it might be more reasonable to try instead to make size an irrelevant feature of the test situation. In theory, this could be accomplished by measuring retention for some experience that does not depend upon spatial (i.e., apparatus) cues and that does not require the organism to learn and remember a highly skilled response. In practice, subjects could be tested for conditioned emotional responding (CER) previously established by off-baseline Pavlovian fear conditioning. With this procedure, the pairing of a conditioned stimulus (CS) with a aversive event takes place in a special situation where no response is required. The effect of this experience is then assessed by presenting the CS in another situation where the subject is engaged in some behavior, such as bar pressing, that is established independently of the conditioning experience. The test apparatus is therefore unrelated to the CS, and the expected effect of the CS, disruption or suppression of the ongoing behavior, does not depend upon the retention of a precisely executed response. If an organism fails to respond to a CS after having been exposed to off-baseline conditioning as an infant, such forgetting is unlikely to be due to perceived changes in the CS or altered response requirements brought about by growth.

Only a few investigators have used CER-like procedures in assessing infantile forgetting (Campbell & Campbell, 1962; Frieman, Warner, & Riccio, 1970; Green, 1962; Snedden, Spevack, & Thompson, 1971), and all have reported retention deficits by the youngest animals tested. However, in all but one of these studies (Campbell & Campbell, 1962), the major variable of interest was learning capacity rather than memory. Thus, two studies (Frieman et al., 1970; Green, 1962) assessed retention only after the subjects were extinguished during stimulus generalization tests, making the retention deficits difficult to interpret. Furthermore, size-change effects were clearly not a consideration. As a result, even in the two studies with no postconditioning manipulations (Campbell & Campbell, 1962; Snedden et al., 1971), no effort was made to prevent apparatus cues from acquiring conditioned strength. Training and testing took place in the same apparatus; thus, poor retention by an animal conditioned as an infant could still be attributed to growth-induced changes in the perception of what may have been highly relevant cues. Only if training and testing took place in different situations could such an interpretation be sufficiently unlikely that infantile forgetting could be attributed to some other factor, such as neurological immaturity.

EXPERIMENT 1

In the first experiment the off-baseline CER procedure was used with rats 20 to 22 days old, an age at which, as shown repeatedly, they forget early learning (e.g., Campbell & Campbell, 1962). To assess the level of original learning, some of these animals were tested immediately after conditioning. It was not clear at the outset whether pups this young could be trained to bar press at a sufficiently high rate to permit one to measure CER in terms of bar-press suppression. Thus, in a real sense, this first study was largely exploratory. Given that CER is demonstrably learned, it was expected that after a prolonged delay the animals would fail to respond appropriately to the CS, thus suggesting that infantile forgetting is not merely a function of physical changes in size.

Method

Subjects

Sixteen albino rat pups of mixed sex born in the Princeton University rat colony were used as subjects. Four pups were randomly selected from each of four litters culled to eight pups in each litter when the animals were 3 days of age.

Apparatus

Two chambers (19-cm long × 20-cm wide × 19-cm high) consisting of two Plexiglas walls, two aluminum walls, and a Plexiglas ceiling were used for Pavlovian fear conditioning. A speaker located on one Plexiglas wall of each chamber delivered an 85-db. 1,600-Hz tone (CS) generated by a Hewlett-Packard audio oscillator (Model 200A). Two Grason-Stadler shock generators (Model E71108) were programmed to deliver in each chamber a .75-sec, 2-mA scrambled shock to the 2-mm stainless-steel grid bar floor.

Eight Scientific Prototype Skinner boxes (24-cm long × 20-cm wide × 19-cm high) were used for assessing the effect of the CS upon bar pressing. The ceiling and two walls of each box were Plexiglass, whereas two walls were aluminum, including one which contained a manipulandum and food cup. The manipulandum (a 4.5 × 3 cm bar) was located 5 cm from the floor in the center of the wall, while a bronze food cup, which protruded into the chamber, was located to the left of the bar just above the 3-mm stainless-steel grid bar floor. A speaker located in the ceiling of each box delivered an 85-db. 1,600-Hz tone generated by the same oscillator used during conditioning.

The conditioning chambers were dark, whereas the Skinner boxes, located in another room, were illuminated by houselights. Recording of data, presentation of stimuli, and the delivery of reinforcement, all were controlled by electromechanical equipment.

Procedure

Bar-press training. All pups were weaned at 16 days of age and subsequently obtained nearly all their food during bar-press sessions. (If its weight dropped to less than 80% of its free-feeding littermates, a pup was allowed to eat freely for an hour after such a session.) After 8 hr. of food deprivation the subjects were each placed overnight (for 15 hr.) inside one of the eight Skinner boxes which were programmed to deliver 20-mg pellets (P. J. Noyes) for bar presses on a variable interval (VI) 15-sec schedule. After the initial overnight session, the pups were then run for the next 2½ days twice daily, each time for 2 hr., with the VI schedule gradually increasing from 15 sec to 90 sec. Thereafter, once the pups were 20 days old, only one 2-hr. VI-90 bar-press session was scheduled each day.

Conditioning. Independently of bar-press training, conditioning began when the pups were 20 days old. Each pup was placed in a conditioning chamber for an hour, during which it was exposed to a 2-min. CS terminated by shock at, 8, 20, 45, and 51 min. into the session. The second day the pups were placed in the other chamber for an hour with tone–shock pairings occurring 5, 31, 37, and 54 min. into the session. On the third day of conditioning, each pup was returned to the original conditioning chamber, and tone–shock pairings occurred 17, 23, 41, and 53 min. into the session.

Testing. When the eight pups from the first two litters (Group I, or immediate) were 23 days old, the 2-min. CS (without shock) was presented for the first time in the Skinner boxes. It occurred four times: 25, 55, 65, and 95 min. into the regularly scheduled 2-hr. bar-press session. When the eight pups from the third and fourth litters (Group D, or delay) were 23 days old, they were transferred to the rat colony where they remained with ad-lib food and water for 35 days. At 58 days of age they were food deprived and over the next 7 days brought down to 80% of their ad-lib weight. Forty-two days after conditioning the rats (now 65 days old) were placed in the Skinner box in which they had been trained, and, as with Group I, the CS alone was presented four times during the 2-hr. bar-press session.

The effect of the CS upon bar pressing was measured by the Kamin suppression ratio (Annau & Kamin, 1961), that is, $a/(a+b)$, where a equals the number of bar presses made during the CS and b equals the number of bar presses made during the 2 min. that precede the CS. A ratio of .50 indicates little, if any, effect of the CS, whereas a ratio of .00 indicates complete suppression of bar pressing during the CS.

Results and Discussion

Bar pressing. All pups learned to bar press rapidly enough for the assessment of CER. On the last day of bar-press training, before the immediate test, rates varied from approximately 200 to 1,800 bar presses per 2 hr. With the first presentation of the CS to Group I, the mean number of bar presses during the 2-min. pre-CS period (i.e., b) was 17. One subject in Group D failed to bar press 42 days later and had to be discarded from the experiment. On the remaining seven subjects, the mean number of bar presses during the first 2-min. pre-CS period (b) was 16.5.

Conditioning. Group I showed nearly total bar-press suppression (seven out of eight subjects with ratios of less than .05) to the first presentation of the CS. A relatively rapid reduction in suppression occurred over the next three trials, such that by the fourth presentation of the CS only three out of eight subjects had ratios of less than .40. The major surprise of the study was that the subjects in Group D also showed almost total suppression (six out of seven subjects with ratios of less than .05) at the first presentation of the CS, 42 days after conditioning. Loss of suppression over the next 3 trials appeared to be somewhat more rapid than in Group I: By Trial 4, only one out of the seven subjects showed a suppression ratio of less than .40. Mann–Whitney U tests indicated that the two groups did not differ from each other on the first two trials, $U(7, 8)=27.5$ and 26.5, although Group D did suppress significantly less than Group I on the last two trials, $U(7, 8)=10.5$ and 11, $p < .05$.

These results indicated that substantial and demonstrable conditioning could be observed with rats as young as 23 days of age even with a bar-press behavioral baseline. They also indicated, unexpectedly, that, even though the delayed animals appeared to extinguish rapidly, they showed virtually no signs of forgetting when the CS was first presented, 42 days after conditioning. These findings seem to suggest that much of the infantile forgetting reported by other investigators for rats of the same age may indeed have been due to the effects of increased size rather than of some neurological deficiency related to LTM. In a situation where size was not likely to have much effect upon performance, forgetting appeared to be minimal.

EXPERIMENT 2

One of the goals of the second study was to test again for the retention of CER by pups conditioned at 20 to 22 days of age. It is possible that the suppression observed 6 wk. after conditioning in the first study was due to some type of unconditioned response to the tone and not to the retention of early conditioning. Thus, in replicating the conditions of the delayed group, it seemed essential to add a control group that at 20 to 22 days of age experienced unassociated tones and shocks. If suppression to tone after a 6-wk. delay were not due to the explicit association of tone with shock, a group that did not experience tone–shock pairings would be expected to respond to the CS no differently than a group that did.

The major purpose of the second experiment, however, was to determine whether CER is forgotten if conditioning takes place at younger ages. Even if size change accounts for the forgetting of events occurring after 20 days of age in the rat, it does not necessarily follow that size change is a major determinant of forgetting by immature rats of all ages. Campbell and Spear (1972) have suggested that the determinants of infantile forgetting may depend upon which stage in development early events occur. By their account, the forgetting of late events could be due to psychological factors, whereas the forgetting of early events could be attributed to neurological deficiencies. Such a two-process account of infantile forgetting implies that, even when size change has no effect upon retention, forgetting will eventually be observed provided that conditioning takes place at a young enough age.

Method

Subjects

Eleven eight-pup litters born in the Princeton University rat colony were subjects. With the first 10 litters, each of the eight pups were randomly assigned to one of eight conditions. The

11th litter was used to increment the number of subjects in two of these conditions.

Apparatus

The same experimental setup as was described in Experiment 1 was used in this study.

Procedure

Conditioning. Prior to weaning, pups were either exposed to three 1-hr. sessions of four tone–shock pairings a day (Treatment P, or paired), as described in the first experiment, or they were exposed to three 1-hr. sessions of four tones and four shocks, where tones occurred on the same schedule as in the P treatment but where shocks occurred randomly four times during the hour with the constraint that no shock occur within 2 min. of the tone (Treatment UP, or unpaired). Two pups from each of the first 10 litters were assigned to the P or the UP treatment at 11 to 13 days of age, two more to one of these treatments at 14 to 16 days of age; two more at 17 to 19 days of age; and finally two at 20 to 22 days of age. Four pups from the 11th litter were exposed to the P treatment at 17 to 19 days, and the other four pups to the P treatment at 20 to 22 days of age. The pups were then weaned at 25 days of age and retained in the rat colony on ad-lib food and water.

Testing. Thirty-two days after treatment, each rat was weighed, food-deprived, and brought down to 80% body weight over the next 6 days. They were then placed in the Skinner boxes for 2 hr. a day receiving 20-mg pellets (P. J. Noyes) on a continuous and then a VI-15 schedule during the first day and on a VI-90 schedule thereafter. Forty-two days after the last day of conditioning, tone was presented four times during the 2-hr. bar-press session on the same schedule as described in Experiment 1. The effects of tone upon bar pressing were again assessed by means of the Kamin suppression ratio as described earlier.

A total of 16 animals died during the course of this study, so that the total number of subjects in each of the eight conditions (treatment plus days of age) at the time of testing was as follows: 9 subjects in P 11–13, 8 in P 14–16, 12 in P 17–19, and 11 in P 20–22; while in UP there were 8 subjects in UP 11–13, 7 in UP 14–16, 8 in UP 17–19, and 9 in UP 20–22.

Results and Discussion

Median suppression ratios for each of the eight groups during each of the four tone presentations 42 days after conditioning are shown in Figure 1. As can be seen, excellent retention of early conditioning was shown by pups trained not only at 20 to 22 days of age but also by pups conditioned at 17 to 19 days of age. Virtually no retention was shown by the two younger age groups. Although pups trained when 17 to 19 days old appeared to suppress slightly less than those conditioned when 20 to 22 days old, this difference was never statistically reliable, even with a slightly larger number of subjects (e.g., on Trial 1, $U[11, 12] = 53$). Both groups suppressed significantly more than their respective control groups across all trials; for pups conditioned at 17 to 19 days, $U(8, 12) = 10.5$, $p < .01$, and for pups conditioned at 20 to 22 days, $U(9, 12) = 3$, $p < .001$. Pups conditioned at 14 to 16 days of age and at 11 to 13 days of age did not differ from their controls on any trial.

These results left little doubt that the suppression observed in the first study in rats conditioned at 20 to 22 days of age was clearly due to the retention of conditioning and not to some unconditioned effect of tone. In Experiment 2 none of the animals in the 20- to 22-day-old UP group, or, indeed, in any of the UP groups, showed any signs of response to tone during the test.

The abrupt difference between the retention abilities of pups conditioned at 17 days of age or older and those conditioned at 16 days or younger was of some interest. If forgetting had been shown to gradually increase as the size of the infant being conditioned decreased, it might have been possible to argue that in some subtle way change in size may have come to affect performance during the retention test. However, the data indicated that forgetting either did or did not occur. This all-or-none effect lends some support to a neurologically determined account of infantile forgetting if only because it is to be expected that a late-developing structure necessary for LTM either would or would not be functional at the time of conditioning. In any event, these data suggest that some kind of two-process account may be necessary to accommodate all the facts of infantile forgetting.

EXPERIMENT 3

It is possible that in the previous study pups 16 days of age or younger did not show signs of retention, not because they forgot, but because they were not conditioned in the first place. A major assump-

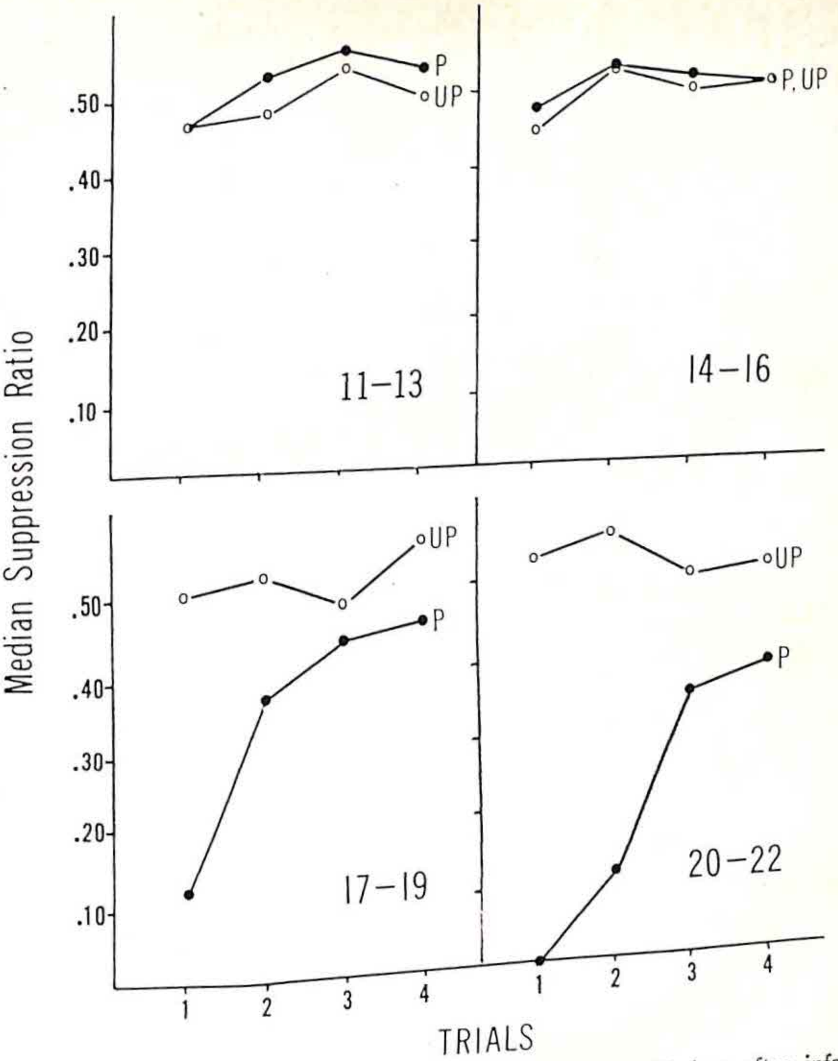

Figure 1. Median suppression ratios on four trials of extinction 42 days after infant rats were exposed to tone–shock pairings (P) or unpaired tones and shocks (UP) when 11–13, 14–16, 17–19, and 20–22 days old.

tion underlying the various accounts of infantile forgetting is that it is strictly a memory phenomenon that is not attributable to deficiencies in learning (e.g., Campbell & Coulter, in press). Therefore, in the third study, pups of 16 days or less, an age which had shown no signs of retention in the second study, were now tested for memory of conditioning at various time intervals after conditioning. If their failure to respond to the CS in the second study was due to a failure in learning, it was expected that no suppression would be observed regardless of whether they were tested soon after conditioning or after substantial delays.

Method

Subjects

Seven eight-pup litters of albino rats born in the Princeton University colony were used as subjects. Each pup was randomly assigned to one of six conditions such that at least one pup from every litter was represented in each condition.

Apparatus

The same experimental setup was used that was described in Experiment 1.

Procedure

Bar-press training. All pups were weaned at 16 days of age and were then trained to bar press as described in Experiment 1. However, training commenced for one third of the subjects when they were 16 days of age, for another third when they were 21 days of age, and for the last third when they were 26 days old.

Conditioning. Independently of bar-press training, half of the pups, when they were 14, 15, and 16 days old, experienced four tone–shock pairings a day on the same schedule as described in Experiment 2 (Treatment P). The other pups at 14 to 16 days of age were exposed to tone on the same schedule as in the P condition, but they were exposed to shock four times daily on a random schedule with no constraints with respect to tone (Treatment RC or random control).

Testing. The effect of tone was again assessed as in the earlier studies by introducing it four times during the regularly scheduled 2-hr. VI-90 bar-press session. For one third of the subjects this occurred 5 days after the last day of conditioning (P-5, $n = 10$) or 5 days after random presentations of tone and shock (RC-5, $n = 9$); for another third of the subjects testing occurred 10 days after conditioning (P-10, $n = 9$) or 10 days after random tones and shocks (RC-10, $n = 9$); and for the last third, testing occurred 20 days later (P-20, $n = 10$; RC-20, $n = 9$). Thus, at the time of testing the pups were 21, 26, or 36 days of age. Retention of conditioning was again assessed by means of the Kamin suppression ratio.

Results and Discussion

Median suppression ratios on the first presentation of tone for each of the six groups are shown in Figure 2. As can be seen, substantial suppression was obtained 5 days after conditioning in Group P-5 (6 out of 10 subjects with suppression ratios of less than .05; all with ratios of less than .30). Somewhat less suppression was observed 10 days after conditioning in Group P-10 (only 1 out of 9 subjects showing ratios of less than .05; 6 with ratios of less than .30). Very little suppression was seen after 20 days (none of the 10 subjects with ratios of less than .05, and only 2 with ratios less than .30). Mann–Whitney U tests indicated that Group P-5 differed significantly from both Group P-10, $U(9,\ 10) = 18$, $p < .05$, and Group P-20, $U(10,\ 10) = 3$, $p < .001$, and that the difference between Group P-10 and P-20 was also significant, at least by a one-tailed test, $U(9,\ 10) = 23$, $p < .05$.

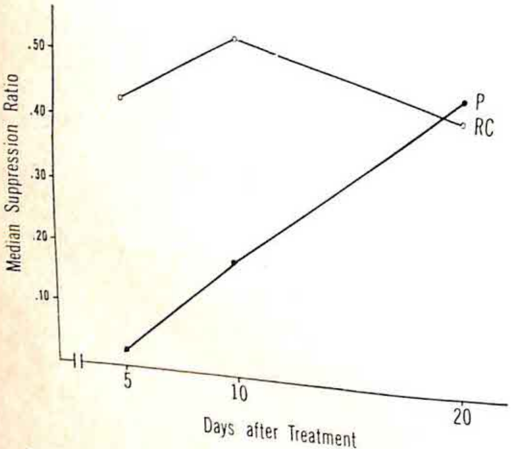

FIGURE 2. Median suppression ratios on the first trial of extinction 5, 10, and 20 days after infant rats were exposed to tone–shock pairings (P) or random tones and shocks (RC) when 14–16 days old.

Overall, no substantial suppression (i.e., ratios of less than .05) was observed in any of the three RC groups, although two subjects in RC-5 and one in RC-20 did show ratios of less than .30 during the first presentation of tone. Nonetheless, both Groups P-5 and P-10 suppressed significantly more than their respective RC groups, $U(9,\ 10) = 4$, $p < .001$, for P-5 versus RC-5; $U(9,\ 9) = 13.5$, $p < .02$, for P-10 versus RC-10, whereas Group P-20 did not, $U(9,\ 10) = 31.5$.

Rate of extinction paralleled these results. Group P-5 suppressed significantly more than RC-5 across all trials, $U(9,\ 10) = 1$, $p < .001$. Group P-10 also suppressed more than RC-10 across all trials, but with greater variability, $U(9,\ 9) = 20$, $p < .05$, one-tailed test. Group P-20, on the other hand, performed no differently than RC-20 across all trials, $U(9,\ 10) = 41$, or on any one trial.

These results clearly indicate that exposing pups to tone–shock pairings at 14 to 16 days of age is sufficient to produce conditioning. Five days after such exposure, the pups responded vigorously to the CS, although within 20 days they appeared to have completely forgotten their earlier conditioning experience. Thus, the retention failure observed in pups conditioned at 14 to 16 days of age in the previous study cannot be solely attributed to the absence of learning, but must reflect true infantile forgetting.

General Discussion

At the outset it was anticipated that if forgetting was observed after using an off-baseline Pavlovian fear-conditioning procedure with young rats, it would be difficult to argue that infantile forgetting is due to growth-induced perceptual–motor changes that might also be expected to affect adult memory. More specifically, if rats were shown unable to retain an experience that would be presumably unaffected by a change in the size of the animal, it would at least be consistent with a neurologically determined account of infantile forgetting. In view of these expectations, the results of these experiments were somewhat surprising. Rats trained when 17–22 days old, an age typically found to be highly susceptible to forgetting, showed substantial retention, suggesting that size change may indeed account for much of the forgetting reported for rats trained at that age. On the other hand, rats conditioned at the younger age of 11–16 days showed no signs of retention after equivalent intervals, suggesting that neurological deficiencies or some unique developmental factor may be responsible for infantile forgetting, after all. The latter forgetting phenomenon, however, appears to occur at a much earlier stage in development than has generally been recognized.

Certainly, the fact that the youngest animals failed to remember does not necessarily mean that the deficiency directly relates to the process of forgetting. It is possible that the absence of retention was only an indirect result of inadequate conditioning (e.g., Underwood & Keppel, 1963) and that for some reason very young rats are less able to acquire a given experience than older rats. Although the data of Experiment 3 indicated that the younger animals did acquire substantial CER, as detected 5 days after conditioning, it is still possible that the present assessments of conditioning are insensitive to existing differences between the age groups. To eliminate the possibility that the younger animals showed poorer retention than older animals because they were less well trained, it would be necessary to show that they would still show poorer retention even when they were conditioned to a demonstrably greater degree than the older animals.

That the older animals did not show retention failures even though such failures have been observed in other CER studies (i.e., Campbell & Campbell, 1962) may not necessarily have been due to the off-baseline procedure presently employed. Certain of the conditioning parameters in this investigation differed from those used elsewhere, for example, in the use of long time intervals between stimuli and between trials. There is some indication in the literature (see Campbell & Coulter, in press) that young animals may require more time for adequate processing of information than adults. It may thus have been a favorable choice of temporal parameters, rather than the presumed irrelevance of size change, that eliminated the retention failures of the older animals. To clearly show that size change is a major determinant of infantile forgetting, it would be necessary to demonstrate with the same conditioning parameters used in this investigation that retention failures by older infants do occur with on-baseline, although not with off-baseline, procedures.

Given that rats conditioned when 17 days of age or older are not likely to forget, the fact that rats conditioned when 16 days old or younger show no signs of retention when they are mature is, in itself and regardless of cause, a phenomenon of some interest. Infantile forgetting must be regarded not as a gradually less important effect but as a process that is sharply dependent upon the age at which the event to be remembered occurs. As was mentioned earlier, the abrupt loss of retention ability in the younger animals is compatible with an account of infantile forgetting that attributes it to the functional absence of some central structure important for learning or memory. The abruptness of this change in retention is made all the more interesting by its similarity to the phenomenon of infantile amnesia in humans. Although data are largely anecdotal (Waldfogel, 1948), it is generally believed that human adults experience almost total amnesia for events occurring before 3 or 4 years of age and have normal memory abilities for events occurring after that age. That an analogous phenomenon was observed in rats may be important for investigators who hope to understand infantile amnesia by studying infrahuman subjects.

The difference between the retention abilities of rats trained when young and those trained when somewhat older is also important because it suggests that infantile forgetting may be multiply determined. As suggested by Campbell and Spear (1972), the forgetting of early experiences may be a two-process phenomenon, wherein determinants important at one age are not important at another age. Thus, the results reported here could be viewed as providing support both for those who have argued that infantile forgetting is due only to perceived changes in the environment (i.e., Perkins, 1965) and for those who have argued that it is due to neurological immaturity (i.e., Campbell, Misanin, White, & Lytle, 1974). Even if both early and late forgetting is ultimately shown to relate primarily to initial learning, the critical variables may still differ depending upon age. Thus, in any future research concerned with infantile forgetting, it will be necessary to carefully specify the age at which learning takes place and to account for the abruptness with which infantile forgetting occurs.

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Asymptotic Reversal Learning in Pigeons: Mechanisms for Reducing Inhibition

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Computer models of asymptotic reversal learning in pigeons were evaluated on the basis of the results of five experiments which defined the asymptotic pattern of within-sessions and between-sessions reversal (Experiment 1) and which provided data on the effects of intertrial interval (Experiment 2), amount of training preceding reversal (Experiments 3 and 4), and intersession interval (Experiment 5). A simple Pavlovian model which assumes that excitation is generated by reinforcement and that temporally unstable inhibition is generated by nonreinforcement is suggested by the between-sessions pattern, but it does not fit the within-sessions pattern. An alternative to temporal decay as a mechanism for eliminating inhibition is the reduction of inhibition by reinforcement. A model based on this assumption fits all of the asymptotic data nicely, as does a noninhibitory model based on the reciprocal assumption that excitation is reduced by nonreinforcement. Temporal decay of inhibition is called into question also as a determinant of the between-sessions pattern.

If extinction is attributed, not to the reduction of an excitatory tendency which has been produced by reinforcement, but to the development of an opposed inhibitory tendency, then some mechanism for reducing inhibition is required. To account for spontaneous recovery, the phenomenon which first led him to the inhibitory theory of extinction, Pavlov (1927) relied on temporal decay. He also suggested that inhibition may be reduced by reinforcement, an idea which has not been taken very seriously in the past, but which now appears to be rather useful in the analysis of reversal learning. The results of the present experiments with pigeons make it clear that the loss of inhibition which must be assumed in order to account for asymptotic reversal performance cannot be explained in terms of temporal decay alone. If nonreinforcment does not diminish excitation, then perhaps reinforcement must be assumed to diminish inhibition.

EXPERIMENT 1: ASYMPTOTIC WITHIN- AND BETWEEN-SESSIONS REVERSAL

The asymptotic pattern of reversal learning in pigeons trained under conditions in which the positive and negative stimuli are reversed *between* sessions is relatively well known: The animals respond readily to both stimuli at the outset of each session and then soon stop responding to the negative (Beale, 1970; Woodard & Bitterman, 1974; Woodard, Schoel, & Bitterman, 1971). The pattern is easily explained on the assumption that excitation remains near its asymptote while inhibition decays in the intersession interval. When reversal takes place *within* sessions, it is not so easy to understand in terms of temporal decay how a precipitous increase in level of responding to the formerly negative stimulus might be brought about, since the intertrial interval affords much less opportunity for decay. Our primary aim in the present experiment was to define the asymptotic within-sessions reversal pattern. For purposes of comparison, between-sessions reversal was studied in the same animals under the same conditions.

Method

Subjects. The subjects were eight male homing pigeons obtained from a local breeder. They were

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maintained at 80% of free-feeding weights on a 24-hr. feeding schedule with a vitamin-enriched diet. Before being put to the present use, they had about eight months of training in a variety of 1-, 2-, and 3-day reversal problems. In the first series of those problems, the discriminanda were diagonal lines; in a second series, the discriminanda were orange and purple key lights. Several different training techniques were used, including, in the second series, exactly the same as that used here.

Apparatus. The animals were trained in a ventilated picnic chest set into a larger sound-reducing enclosure. Centered on one wall of the subject's compartment was a single pecking key on which the discriminanda—white triangle and white X—could be projected; a heavy-duty relay mounted behind the wall and operated by each peck provided auditory feedback. Directly below the key was a feeder, a grain-filled, motor-driven box, normally retracted from the animal's compartment (its opaque front face flush with the wall), which could be inserted into the compartment for a period of 2 sec during which it was illuminated with white light. All events of the training were controlled by conventional relay equipment, and responses were recorded automatically on a printing counter.

Procedure. A discrete-trials/fixed-interval training technique (Woodard & Bitterman, 1974) was employed. Each trial began after an intertrial interval (ITI) of 10 sec in darkness with illumination of the key by a positive or a negative stimulus (S+ or S−). All responses to the stimulus were counted during the first 20 sec (the fixed interval), after which the counting stopped although the key continued to be illuminated. In S+, the next response produced reinforcement—the key light turned off and the illuminated tray of grain was presented—after which the next intertrial interval began. In S−, the programmer then started a 10-sec penalty timer which was reset by each subsequent response, and the trial ended only when the penalty timer timed out; that is, response to S− was penalized by postponing its termination. Each stimulus appeared equally often in each session, with not more than three S+ or three S− trials in succession. In any session, each of the stimuli was S+ for four animals and S− for the four others. Four quasi-random orders of stimuli were used which made it possible to plot properly balanced trial-by-trial group learning curves.

The animals were trained first in a series of 20 2-day within-sessions reversals. Fifty trials were given in each daily session except in the very first, when there were only 30 trials. In the third session and in each subsequent odd-numbered session, positive and negative stimuli were reversed after the first 20 trials. In each even-numbered session, the positive stimulus was the same as that in the last 30 trials of the preceding session and in the first 20 trials of the following session. Training was continued until statistical tests showed performance in the last six reversals to be stable, and pooled performance in those six reversals was taken as asymptotic. Then the animals were trained in a series of 12 2-day between-sessions reversals, achieving asymptotic performance in the last six. There were 50 trials in each daily session, with positive and negative stimuli reversed at the outset of each odd-numbered session. All other features of the procedure were the same as in the within-sessions reversal training.

Results

Despite the fact that the animals came to this experiment with a good deal of prior reversal training, they showed marked progressive improvement, an outcome which suggests that an important component of improvement in reversal is stimulus specific. Description and analysis of the course of improvement are reserved, however, for a subsequent paper, since our strategy is first to try to understand the asymptotic performance and then to test any reasonable interpretations in terms of a broader range of data. The points plotted in Figure 1 show the mean asymptotic within-sessions and between-sessions performance.

The between-sessions pattern is quite like that which has already been described. The within-sessions pattern resembles the between-sessions pattern on Day 2 (the nonreversal day). The unique feature of the within-sessions pattern lies in the rapid crossover of the curves on the postreversal trials of Day 1. On the assumption of intersession decay of inhibition, the decline of responding to the new S− may be attributed to the development of inhibition. The increase in responding to the new S+ may be attributed in part to development of excitation—on the assumption that there also is some decay of excitation in the intersession interval which is obscured to a certain extent by a performance ceiling—and in part to intertrial decay of inhibition. A preliminary attempt to fit the within-sessions data with a decay model gave good results on the assumption of rapid intertrial decrement of inhibition. To test this assumption, an experiment was required in which ITI was varied over a wider range.

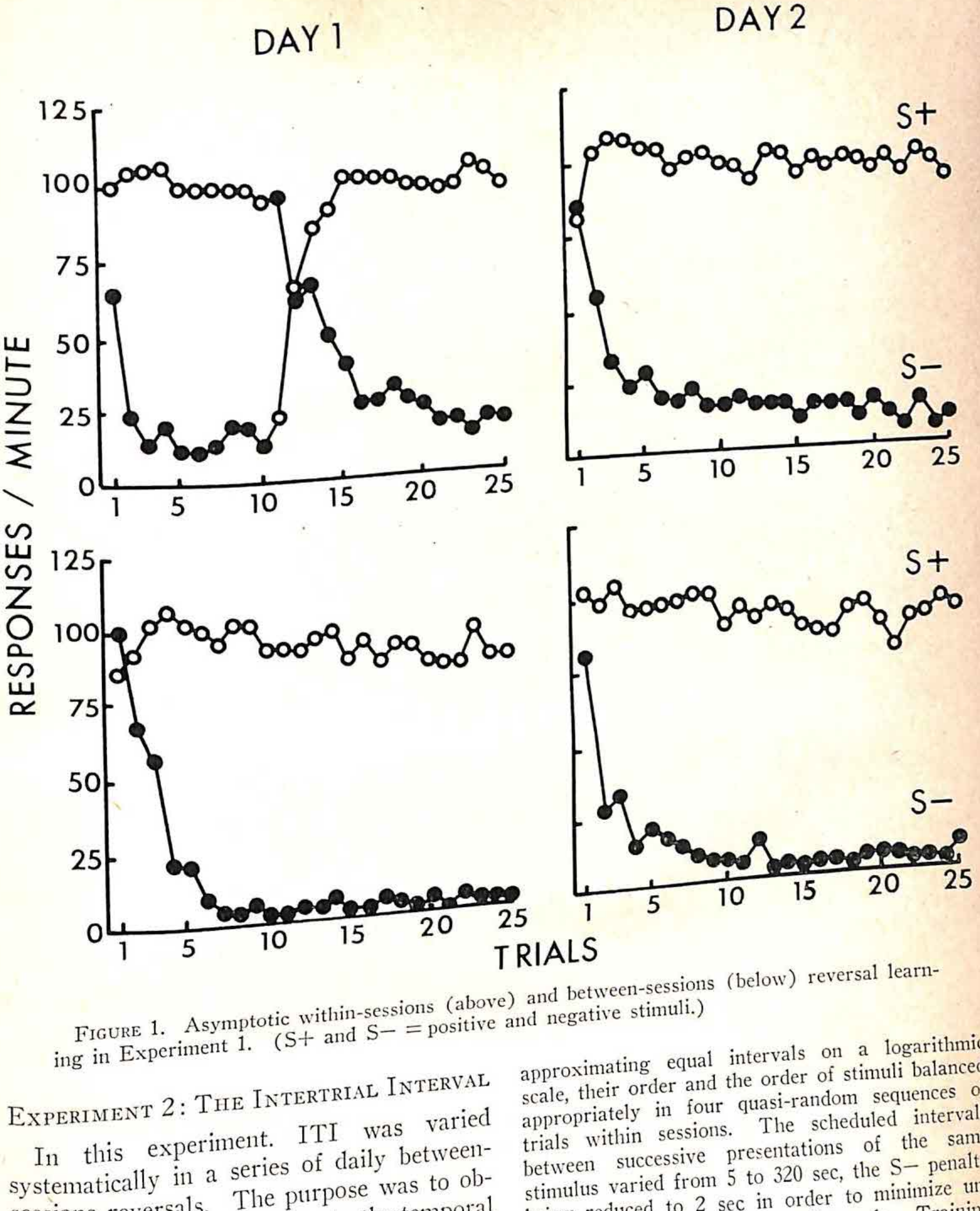

FIGURE 1. Asymptotic within-sessions (above) and between-sessions (below) reversal learning in Experiment 1. (S+ and S− = positive and negative stimuli.)

EXPERIMENT 2: THE INTERTRIAL INTERVAL

In this experiment. ITI was varied systematically in a series of daily between-sessions reversals. The purpose was to obtain further evidence relating to the temporal decay of inhibition.

Method

The subjects and the apparatus were the same as in the first experiment. The animals were trained in a series of daily reversals (positive and negative stimuli reversed at the start of each session) with 48 trials in each daily session. There were four different ITIs—5, 14, 40, and 117 sec—approximating equal intervals on a logarithmic scale, their order and the order of stimuli balanced appropriately in four quasi-random sequences of trials within sessions. The scheduled intervals between successive presentations of the same stimulus varied from 5 to 320 sec, the S− penalty being reduced to 2 sec in order to minimize uncontrolled variation in these intervals. Training continued until statistical tests showed performance to have stabilized. Performance in the last six reversals was taken as asymptotic.

Results

Plotted in Figure 2 are points based on the pooled data for one of the four different orders of stimuli and intervals used in the experiment. Despite the huge variation in ITI, there was relatively little in S− performance, as can be seen in Figure 2 where vertical lines proportional in length to the intervals between successive S− trials are interposed between the relevant S− data points. The correlation of rate of response and interval between successive S− presentations for the last 15 S− trials is negligible ($r(13) = -.096$, $p > .1$). The results for the other three orders are the same. It should be clear, then, that the rapid increase in response to the postreversal S+ found in the within-sessions data cannot be dealt with in terms of intertrial decay of inhibition alone, since the far-longer ITIs of the present experiment did not produce comparable increases in rate of responding to S−. The only way to save a simple decay interpretation given these data is to assume that increase in response to the postreversal S+ is due in substantial part to increase in excitation, which in turn requires the assumption that there has been some intersession decay of excitation. The objection that no such decay is evident in responses to S+ on early trials of the session can be met by assuming a substantial performance ceiling. Promising results in preliminary attempts to model the data of the first two experiments with inclusion of the ceiling assumption led us to look for independent evidence of a ceiling.

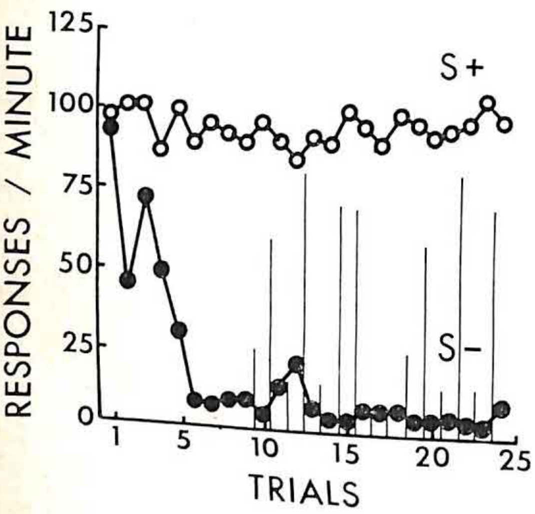

FIGURE 2. The data for one of the four real orders of Experiment 2 in which intertrial interval was varied widely. (The vertical lines show the intervals between successive S− presentations after performance had fallen to the baseline. The shortest interval in this set of data was 14 sec and the longest was 208 sec. S+ and S− = positive and negative stimuli.)

EXPERIMENTS 3 AND 4: THE CEILING EFFECT

In these experiments, amount of training preceding reversal was varied. The purpose was to determine whether the continued reinforcement of S+ beyond the point at which response to it becomes maximal increases the difficulty of reversal.

Method

The subjects and apparatus were the same as before. In Experiment 3, the animals were trained in a series of 18 2-day within-sessions reversals with number of trials preceding reversal varied systematically. Each odd-numbered session consisted of either 10 or 40 trials (in balanced order) followed by 10 trials with positive and negative stimuli reversed. Each even-numbered session consisted of 50 trials with the stimuli differentially reinforced as in the preceding session. In Experiment 4, the animals were trained in a series of nine between-sessions reversals with the number of sessions preceding reversal—either one or three in balanced order—varied systematically. There were 50 trials per session. In both experiments, positive and negative stimuli were scheduled according to four balanced quasi-random orders, and the S− penalty was 3 sec.

Results

The results of Experiment 3 are plotted in Figure 3, the data for the two conditions superimposed in such a way that postreversal curves overlap. Analysis of variance shows the effect of number of trials preceding reversal on postreversal performance (Day 1) to be of borderline significance, $F(4, 28) = 2.47$, $p = .048$, for the Number of Prereversal Trials × Stimuli × Postreversal Trials interaction. This outcome seems primarily due, not to a ceiling effect, but to a difference in the postreversal S+ curves attributable to the fact that 10 prereversal trials were not enough to bring the S− performance to asymptote. The effect of number of prereversal trials on post-

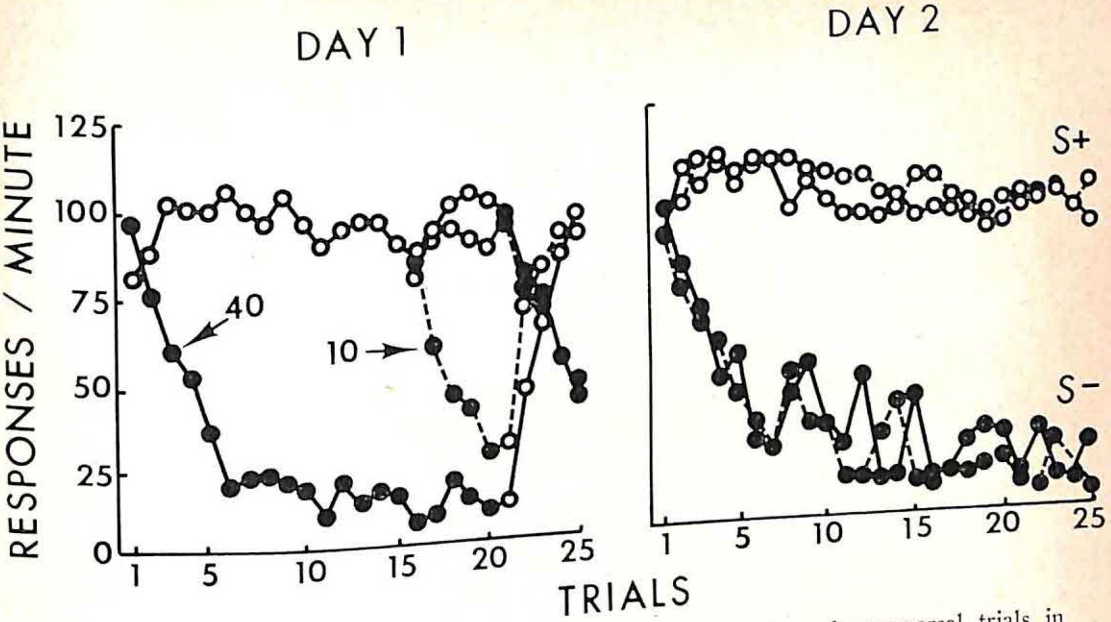

FIGURE 3. Within-sessions reversal as a function of the number of prereversal trials in Experiment 3. (S+ and S− = positive and negative stimuli.)

reversal S− performance is negligible ($F < 1$). The results of Experiment 4 are plotted in Figure 4. Here the effect of amount of prereversal training is significant—$F(4, 28) = 5.27$, $p = .013$, for the Number of Prereversal Days × Stimuli × Trials interaction—and the existence of a ceiling is indicated by slower decline in response to S− after 3 days of prereversal training. The magnitude of the effect seems too small to support the decay interpretation, but whether it is or not can be determined only by a fitting procedure. The procedure and outcome will be described after the results of a further experiment on an important feature of the model have been considered.

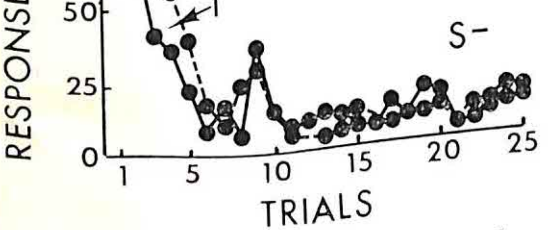

FIGURE 4. Between-sessions reversal as a function of the number of prereversal sessions in Experiment 4. (S+ and S− = positive and negative stimuli.)

EXPERIMENT 5: THE INTERSESSION INTERVAL

Construction of a formal model requires a decision as to whether the intersession decay of excitation and inhibition follows the same course as the intertrial decay, or whether factors other than time (such as handling or change in experimental conditions) contribute to intersession decrement. This experiment was designed to provide a rough estimate of the rapidity of the change.

Method

The subjects and apparatus were the same as before. The animals were trained in 11 50-trial sessions with positive and negative stimuli reversed at the start of each session. Five of the reversals were scheduled after an intersession interval of 25 min. and five after an intersession interval of 1 day.

Results

The results plotted in Figure 5 show better reversal after the shorter intersession interval (ISI), an obtained difference opposite in direction to that predicted on the assumption of intersession decay. The results suggest that intersession loss may not be due to temporal decay at all, or, if it is, that the decay is so rapid as to be virtually complete by 25 min. Informal study of reversal performance following even shorter ISIs (removing the pigeon from the experimental apparatus and then immediately replacing it to begin the next reversal session) indicates much the same rapid reinstatement of response to the former S− as is shown in Figure 5. These results also argue strongly against an explanation in terms of the temporal decay of inhibition although it should be noted that an earlier *choice* experiment with pigeons showed substantial retention of preference in sophisticated pigeons tested 20 min. after training (Gonzales, Behrend, & Bitterman, 1967). In pigeons with no reversal training, of course, retention is extremely good even after 2 weeks (Behrend, Powers, & Bitterman, 1970). It must remain for future experiments to clarify the basis of the intersession loss. For the present, our formal model simply distinguishes intersession from intertrial loss and makes no attempt to deal with its mechanism.

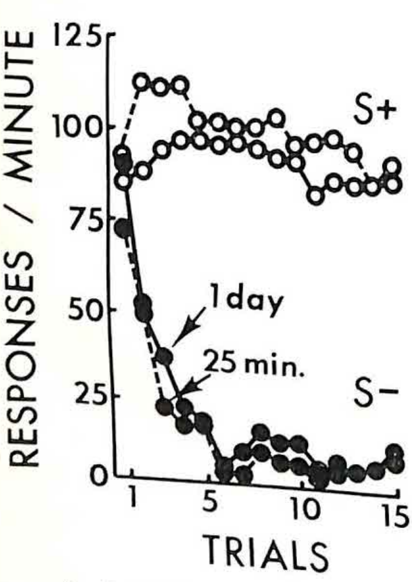

FIGURE 5. Between-sessions reversal as a function of intersession interval in Experiment 5. (S+ and S− = positive and negative stimuli.)

DISCUSSION

The first question to be considered is whether the data reported here can be dealt with in terms of a decay model, which now is specified: Simple asymptotic growth functions are assumed for excitation (E) and inhibition (I). The growth of E ($0 \leq E \leq 1$) on reinforced trials is described by the incremental equation

$$E_m = E_n + g_E(1 - E_n) \qquad (1)$$

and the growth of I ($0 \leq I \leq 1$) by

$$I_m = I_n + g_I(1 - I_n) \qquad (2)$$

where E_n and $I_n = E$ and I at the outset of trial n, E_m and $I_m = E$ and I at the termination of trial n, and g_E and $g_I =$ excitatory and inhibitory growth rates ($0 \leq g \leq 1$). Both E and I are assumed to decay exponentially in the intertrial interval according to the incremental equations

$$E_{n+1} = t_E E_m \qquad (3)$$

and

$$I_{n+1} = t_I I_m, \qquad (4)$$

where E_{n+1} and $I_{n+1} = E$ and I at the outset of trial $n + 1$, and t_E and $t_I =$ proportion of E and I remaining after the intertrial interval ($0 \leq t \leq 1$). Both E and I are assumed also to decline in the intersession interval according to the incremental equations

$$E_i = s_E E_f \qquad (5)$$

and

$$I_i = s_I I_f, \qquad (6)$$

where E_i and $I_i = E$ and I at the outset of the initial trial of a session, E_f and $I_f = E$ and I after the final trial of the preceding session, and s_E and $s_I =$ proportion of E and I remaining after the intersession interval ($0 \leq s \leq 1$). The use of separate intertrial and intersession decrements rather than a single decay function is dictated by the results of Experiment 5. Performance is assumed to be determined by E and I according to the following equation:

$$R_n = p(E_n - aI_n), \tag{7}$$

where R_n = rate of response on trial n, p = performance scaling factor, and a = asymptotic adjustment factor. Since E and I are normalized, a provides for the possibility that their effective asymptotes may be different. Finally, the possibility of a limit on rate of responding is provided for in the following truncation rule:

$$0 \leq R_n \leq c, \tag{8}$$

where c = performance ceiling. The magnitude of the ceiling effect is given by $p - c$.

We have here chosen to use the simplest asymptotic growth and decay functions, although they might well prove, say, to be sigmoidal, and our performance function also is simple, although it might well prove to be nonlinear in E and I. Whether or not these assumptions are correct will become evident only as the constraints on the model are increased in its application to a broader range of data. At the same time, the range of constructs will almost surely have to be extended, as, for example, to provide for the generalization of E and I when discriminability of the stimuli is reduced. (Generalization equations contributed nothing to fits of the data dealt with here and were therefore discarded.) The omission of any reference to stochastic processes is certain to be noticed by readers familiar with computer models of behavior. In order to focus on mechanisms critical to the understanding of reversal performance, we have chosen to work with a deterministic model and not, for the moment, to concern ourselves with sources of variability such as individual differences and errors of measurement.

To fit the experimental data, the foregoing equations were coded in FORTRAN and evaluated on each of an appropriate sequence of S+ and S− trials. Parameter estimates were made according to a least-squares criterion. Since the set of difference equations did not permit use of an analytical least-squares technique, the parameters were estimated by an iterative nonlinear least-squares procedure—an interactive version of the program described by Daniel and Wood (1971) based on the Marquardt (1963) maximum neighborhood method combining the Gauss method and the method of steepest descent. On each iteration, initial values of E and I were themselves iteratively determined on the assumption that they must be the same (at asymptote) for reversal r as for reversal $r + 2$. The program provided estimates of parameters and their standard errors, estimates of the independence of the parameters (parameter intercorrelations), and a measure of the goodness of fit of the model to the data (root mean square, *RMS*, error in units of responses per minute) adjusted for the degrees of freedom lost by adding parameters. The parameter-estimating procedure was terminated when either the sums of squared deviations or the parameter estimates changed by a factor of less than 10^{-6}.

Not all of the assumptions of the model are required by the between-sessions data. For example, a good fit to the mean between-sessions reversal curves of Figure 1 is achieved with only three free parameters, one (g_I) governing the growth of I, a second (s_I) governing its decay between sessions, and a third (p) for scaling performance. No growth or decay of E need be assumed, no intertrial decay of I, and no inequality of E and I asymptotes; the results are summarized in row 1 of Table 1. Permitting the ceiling and asymptotic factors to operate does not produce a markedly better fit ($RMS = 6.1$), although the improvement is statistically significant, $F(4, 92) = 8.4$, $p < .01$. Clearly, the between-sessions data make no great demand upon the model and require no mechanism for the reduction of I other than intersession decrement.

A good fit of the model to the within-sessions data also could be achieved but only at the expense of high intercorrelation among parameters, requiring the conclusion that these data do not in themselves provide sufficient constraint on the parameters to afford a suitable evaluation of the model.

TABLE 1

PARAMETER ESTIMATES AND RMS ERRORS

Row	g_E	g_I	t_E	t_I	s_E	s_I	p	a	c	d	*RMS*
1		.35				0	92				6.9
2	1	.65	.9970	.967	.91	0	184	.84	96		11.3
3	.3	.34			.91	0	97.5	.81		.3	8.6

Note. The number of decimal places entered is in each case appropriate to the standard error. Zero represents a value of less than 10^{-5}. The unit of measurement for p, c, and *RMS* (root mean square) is responses per minute. See text for further description.

Accordingly, an attempt was made simultaneously to fit three sets of data: (a) the data for one of the four real orders of stimuli used in the within-sessions training of Experiment 1 (25% of the data shown in the upper portion of Figure 1); (b) the data for one of the four real orders of stimuli and ITIs used in Experiment 2 (the data shown in Figure 2); and (c) the mean-order ceiling data of Experiment 4 (the data shown in Figure 4). (It should be noted that real-order data points show more variability than mean-order points since they are based only on 25% of the data, and the *RMS* values, which are corrected for the number of data points but not their reliability, therefore are increased correspondingly.) The results are summarized in row 2 of Table 1, and the model curves for the real-order within-sessions reversal data of Day 1 are shown in Figure 6. Clearly, the fit is not very good. The increase in response to the postreversal S+ is much too slow; response to the prereversal S− declines too rapidly and too far; and response to the postreversal S− does not decline far enough. The model curves corresponding to the data of Experiment 2 in which ITI was varied more widely (not shown) also reveal a substantial discrepancy which takes the form of trial-to-trial fluctuations in S− performance both much too large and inconsistent with fluctuations in the data. The crux of the difficulty seems to be that while the within-sessions reversal data call for substantial intertrial decrement in I, the ITI data do not permit such decrement except on the assumption of a substantial ceiling, but the latter assumption is not supported by the results of the ceiling experiments.

Two subsidiary problems are evident in row 2 of Table 1. One is that g_E is set equal to 1, but that may simply mean that the largest loss in E (9% over the intersession interval) is recovered after only a single reinforcement. Data on early training would provide more constraint on the determination of the parameter. A more serious problem is that s_E is much too large in relation to t_E to warrant the assumption of a single temporal decay function. While a separate decay parameter was introduced to allow for the operation of other sources of decrement in the ISI, these results, if taken seriously, would imply that there is some mechanism which tends to preserve E during the ISI. The only alternative assumption, with all of its attendant difficulties, is that E decays to a nonzero asymptote.

The poor fit obtained with the decay model suggests that some other mechanism for reducing I must be found. In the quest for such a mechanism, it will surely be necessary to go beyond disinhibition—the disruption of inhibition by novel or intense stimuli—which does not explain asymptotic within-sessions reversal performance. Even if the animal is disturbed by reversal in the early postreversal trials and the I associated with the postreversal S+ is therefore disrupted, the disturbance soon should subside (as the animal adjusts to the new contingencies) and response to S+ should decline once more, which it does not. Furthermore, the I developed by the postreversal S− also should be disrupted, preventing the rapid decline of response to it which is observed. Clearly some permanent mechanism is required, such as the reduction of inhibition by reinforcement (RDI). On this assumption, the effects of reinforcement and nonreinforcement are no longer completely independent, although neither are they so closely related that the distinction between E and I is lost. While changes in E and I strongly covary on reinforced trials, the theory still permits independent reduction of I in time or by disturbing events and so continues to provide ready explanations of such phenomena as spontaneous recovery and disinhibition. The mechanism of reinforcement-decremented inhibition may be incorporated in the model by adding the equation

$$I_m = dI_n, \qquad (9)$$

where $d =$ proportion of I remaining after reinforcement. Equation 9 is applied following reinforcement on each S+ trial and thereby reduces the I associated with whatever stimulus is reinforced.

The results of the simultaneous fit of the RDI model to the same three sets of data used in the evaluation of the decay model are given in row 3 of Table 1. A good fit is obtained even with no intertrial decay at all and with no ceiling. (The inclusion of the ceiling parameter does not significantly reduce the *RMS*, although the small but reliable effect found in Experiment 4 suggests that the ceiling assumption may be necessary to the model if a high level of precision is to be achieved.) The value of d is not closely determined, but that probably is due to the fact that d is strongly constrained only by part of the within-sessions reversal data. Model curves for the real-order within-sessions reversal data on Day 1 are shown in Figure 6. With the possible exception of a few early S− points, they reflect the data faithfully, as do the curves fitted to the data of the ITI and ceiling experiments. There are no grounds, then, for questioning the assumption that I is reduced by reinforcement.

While the hypothesis that reinforcement reduces I fits the asymptotic data which have resisted analysis in terms of intertrial decay, its appropriateness to a broader range of data—as, for example, on approach to asymptote—remains to be determined. At the same time there might well be some

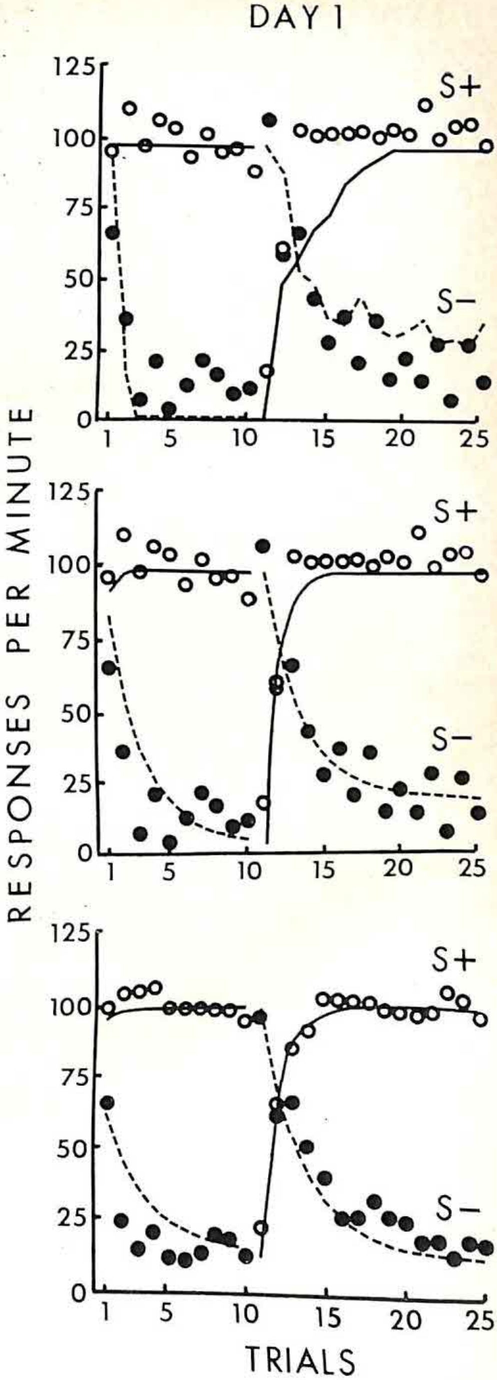

FIGURE 6. Fits of three models to the Day 1 within-sessions reversal data of Experiment 1. (Above: decay model fitted to the data for one of the real orders; center: RDI [reduction of inhibition by reinforcement] model fitted to the same data; below: E-only model fitted to the mean-order data. S+ and S− = positive and negative stimuli.)

further consideration of the reciprocal hypothesis, rejected early by Pavlov, that nonreinforcement reduces E, an assumption which may permit us to dispense with the concept of inhibition altogether. It is interesting to note that Wagner and Rescorla (1972) recently have been able to model a variety of phenomena such as those which first suggested the concept of inhibition on the assumption of a single excitatory process strengthened by reinforcement and weakened by nonreinforcement, athough only by permitting it to take on negative values, and one may argue, of course, that negative E is no different in principle from I. In any case, we have considered the appropriateness of an E-only model (with E never less than zero) and have written a preliminary version which fits the asymptotic within-sessions reversal data well.

It is assumed that E $(0 \leq E \leq 1)$ is incremented toward an asymptotic value by each reinforcement and decremented toward some minimal value (attributable to sensitization) by each nonreinforcement according to the equations

$$E_m = E_n + g_R(1 - E_n) \qquad (10)$$

and

$$E_m = E_n + g_N(b - E_n), \qquad (11)$$

where $E_n = E$ at the outset of trial n, $E_m = E$ at the termination of trial n, $g_R =$ excitatory growth rate $(0 \leq g_R \leq 1)$, $g_N =$ excitatory weakening rate $(0 \leq g_N \leq 1)$, and $b =$ minimal value of E. The performance function then is simply a matter of scaling E to the appropriate units:

$$R_n = pE_n, \qquad (12)$$

where $p =$ performance scaling factor. To account for the increase in response to S− immediately following the ISI, we assume intersession reversion of E toward a residual value—a sort of long-term memory concept:

$$E_i = E_f + s(r - E_f), \qquad (13)$$

where $E_i = E$ at the outset of the initial trial of a session, $E_f = E$ after the final trial of the preceding session, $s =$ rate of intersession decay toward the residual, and $r =$ residual value of E. Without discussing the model in more detail, we show in the lower portion of Figure 6 the curves resulting from a fit to the mean-order within-sessions reversal data of Experiment 1—since order is not critical in this model, we can take advantage of the greater stability of the mean data—simply to demonstrate the general feasibility of the noninhibitory approach (parameter values: $g_R = .58$, $g_N = .32$, $b = .15$, $p = 99$, $s = .60$, $r = .93$; RMS error $= 6$). Further experiments will be required to determine whether the concept of inhibition is useful at this level of description.

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Surprise Value of Food Determines Its Effectiveness as a Reinforcer

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In Experiment 1, naive pigeons were pretrained on a discrimination procedure. In the presence of a colored houselight (S+), the birds received free food on a variable time schedule. The S+ alternated with a houselight of a different color (S−) in which food was withheld. All birds were then autoshaped with a white keylight. One group (positive) was autoshaped in the presence of S+; one (negative) was autoshaped in S−; and one (novel), in a houselight color never previously seen by these animals. The negative group responded more to the keylight conditioned stimulus than did the novel or positive groups. In Experiment 2, a magazine-trained control was added, and the number of pretraining sessions was reduced. The positive group responded less than the other groups. The results supported the hypothesis that the reinforcing effectiveness of an unconditioned stimulus increases with its surprise value.

Kamin (1969, p. 293) offered the influential suggestion that the effectiveness of an unconditioned stimulus (US) as a reinforcer depends on the extent to which its occurrence is surprising to an animal. Kamin assumed that the degree of surprise occasioned by an event depends, in turn, on the stimulus situation in which that event occurs. An animal will not be surprised by an event whose occurrence is predicted by the cues available at the time of the event. It will be surprised by an event whose occurrence is not predicted by the available cues. Later writers (Terry & Wagner, 1975; Wagner, Rudy, & Whitlow, 1973) have assumed that an animal will also be surprised by an event whose certain *non*occurrence is predicted by the available cues.

Evidence supporting the surprise hypothesis has been produced by a number of experiments (Kamin, 1969; Terry & Wagner, 1975; Wagner, 1971, pp. 198–199; Wagner et al., 1973). However, the majority of experiments that bear most closely on the hypothesis have used electric shock as the US. A general hypothesis of associative learning should be based on the broadest possible foundation of empirical evidence, including confirmation in both aversive and appetitive situations. For this reason the present experiment was designed to test the surprise hypothesis using a procedure in which the US was the presentation of food.

All subjects, naive pigeons, received identical treatment in the first phase of the experiment (pretraining). Throughout pretraining the pecking key in the experimental chamber remained dark. In the presence of a colored houselight (S+), the birds were given 4-sec access to grain (the US) independently of responding, on a variable time (VT) 60-sec schedule. The S+ alternated with a houselight of a different color (S−), in which the US was withheld.

In the second phase of the experiment (training), all birds were exposed to pairings of a 10-sec white keylight and the US. The white keylight was the target conditioned stimulus (CS), and the amount of autoshaped responding to the lighted key (Brown & Jenkins, 1968) was the measure

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of conditioning. Before beginning the training phase, the birds were randomly divided into groups. One group of birds (positive) was autoshaped with the chamber illuminated by S+, the colored houselight in which the US had been delivered in pretraining. A second group of birds (negative) was autoshaped with the chamber illuminated by S−, the colored houselight in which the US had been withheld in pretraining. In the training sessions, the houselight color did not alternate; it remained S+ for the positive group and S− for the negative.

The surprise hypothesis predicts that the birds autoshaped in the presence of S− will respond more to the keylight CS than those autoshaped in the presence of S+. The S− signals that the US will not occur, thus the arrival of the US in the presence of S− will be surprising. The S+ signals that the US may occur at any time, and the delivery of the US in S+ will not surprise the animals. Because it is more surprising in S− than in S+, the US will be a more effective reinforcer in the presence of S−.

A third group of animals was also run. This group (novel) was autoshaped with the chamber illuminated by a colored houselight never previously seen by these animals. Prior to being autoshaped in the novel houselight color (S^N), the novel group received the same discrimination pretraining with S+ and S− given to the positive and negative groups. This group was included in an attempt to assess the control of background cues in relation to the control exerted by S+ and S−.

EXPERIMENT 1

Method

Subjects

The subjects were 41 experimentally naive White Carneaux pigeons, maintained at 80% of their free-feeding weights. Two birds were dropped from the study because of equipment failures, and 3 were dropped because of failure to meet a pretraining criterion. The data reported are from the remaining 36 birds.

Apparatus

The pigeon chamber was enclosed in a Grason-Stadler sound-attenuating chest. Masking noise from a speaker supplemented the noise of the ventilating fan. The intelligence panel was curved, being 13 cm further from the opposite wall at the middle than at the corners. A Lehigh Valley grain magazine was located in the middle of the intelligence panel. A photocell was mounted on one side of the magazine, and a photocell light was mounted on the other. The photocell light also served as a magazine light. A pecking key mounted directly above the grain magazine could be transilluminated with a white light. Two side keys remained covered throughout the experiment. The section of ceiling created by the outward curve of the intelligence panel was made of metal. The rest of the ceiling was formed by a 20 × 35 cm sheet of white translucent Plexiglas, which was flush with the metal section. Above the Plexiglas ceiling were three evenly spaced red, three blue, and three green Christmas tree light bulbs. These illuminated the chamber with a diffuse red, blue, or green glow. The recording and programming equipment were in an adjacent room.

Procedure

Magazine training. The magazine training procedure was completely automated. On the first day of magazine training the chamber was completely dark between magazine presentations. When the grain magazine was up the photocell light was on; this light provided the illumination by which the bird ate. The grain magazine was up when the bird was placed in the box. When the bird put its head in the grain magazine to eat, the photocell beam was intercepted. As soon as the beam had been intercepted for a cumulative total of 5 sec, the magazine presentation was terminated and the photocell light was turned off. This procedure ensured that the bird ate a full 5 sec each time the magazine was presented, regardless of how long it paused before begining, or during, its cumulative 5 sec of eating. After a fixed interval of 15 sec the magazine was automatically presented again. The first session ended after 30 magazine presentations. On the second day, the interfood interval was increased to 30 sec, whereas magazine duration was decreased to 4 sec and session length to 24 food presentations. On the third day of magazine training the chamber was continuously illuminated with the positive houselight (S+). The magazine was automatically presented after a variable interval of 60 sec from the end of the previous presentation. Each presentation terminated after the bird had eaten for a cumulative total of 4 sec, and the session ended after 24 presentations.

Pretraining. In pretraining, as in magazine training, the pecking key was never illuminated. In this phase, the negative houselight (S−) was introduced. The S+ and S− alternated in a Gellerman series in which the same stimulus was repeated a maximum of three times. There were 12 presentations of each stimulus in a session.

The durations of S+ and S− were always 90 sec. A 10-sec interstimulus interval (ISI), in which the chamber was completely dark, followed each 90-sec stimulus. In the presence of S+, the grain magazine was presented on a VT 60-sec schedule. The birds were given a median of 18 grain presentations per day (range = 17–19). In this and in the next phase of the experiment, the duration of magazine presentation was a fixed 4 sec, regardless of whether the animal ate or not. However, eating was monitored by the photocell, and the number of food presentations during which the animal ate was recorded. In the presence of S−, the grain magazine was never presented (extinction). The session length was 40 min. Each bird was given seven pretraining sessions that were conducted on successive days.

The birds were given a median total of 126 food presentations in the pretraining phase (range = 123–127). A bird was dropped from the experiment if it ate during fewer than 100 presentations. Three birds were dropped after pretraining for failure to meet this criterion. The median total of food presentations during which the animal ate for the 36 birds that completed the experiment was 122 (range = 105–126).

Training (autoshaping). In the training phase the birds were run on an autoshaping procedure. The autoshaping trials were the same for all subjects. A trial began with the transillumination of the pecking key with a white light. After 10 sec the keylight was terminated and the grain magazine was presented for 4 sec. The presentation of grain was completely independent of responding on the key. The termination of the magazine presentation initiated a variable intertrial interval having a mean of 60 sec. The birds were given a median of 32 autoshaping trials per session (range = 30–34). Electromechanical counters recorded the total number of pecks to the lighted key and the number of trials to the first peck.

Before beginning the training phase, the birds were randomly assigned to three groups of 12 each. The only difference between the three groups was the houselight stimulus that illuminated the chamber during the autoshaping sessions. Only one houselight stimulus was presented to each group. The positive group was autoshaped in S+, the houselight that had signaled the VT 60 sec in pretraining; the negative group in S−, the houselight that had signaled extinction, and the novel group in a houselight color that had never been seen by these animals.

As in the pretraining phase, the houselight stimulus was always presented for 90 sec, and each presentation was followed by a 10-sec ISI in which there was no houselight. The film reader that programmed the autoshaping trials ran only when the houselight was on, so that an autoshaping trial was never initiated during an ISI. However, if an autoshaping trial was in progress when an ISI began, the trial was completed in the ISI. The houselight stimulus appropriate to each group was repeated 24 times in each session; thus, the session length was the same (40 min) as it had been in pretraining. The autoshaping training sessions were conducted on three successive days.

The colored houselight stimuli were completely counterbalanced. In the pretraining phase, six birds were run in each of the six possible houselight color combinations (green, G; red, R; and blue, B): G(+)R(−), R(+)G(−), G(+)B(−), B(+)G(−), R(+)B(−), and B(+)R(−). In the training phase, two birds from each of these six groups were autoshaped in the red houselight, two in the green, and two in the blue. This created a total of 18 subgroups of two birds per subgroup. There were 6 positive subgroups, 6 negative, and 6 novel. For example, of the six birds pretrained in G(+)R(−), two were autoshaped in green to form one positive subgroup, two were autoshaped in red to form one negative subgroup, and two were autoshaped in blue to form one novel subgroup.

Results

The measure of conditioning used was the number of key peck responses per trial. Figure 1 shows the mean number of key peck responses per trial of each group on each of the 3 days of autoshaping.

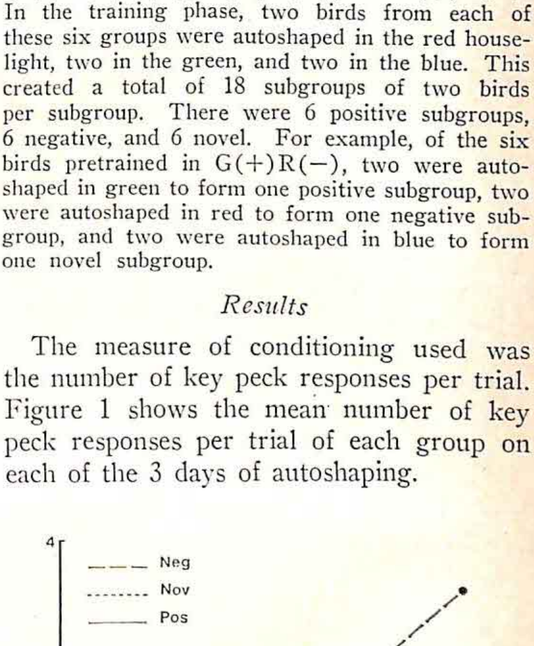

FIGURE 1. Key peck responses per autoshaping trial. (The mean scores of the three groups of Experiment 1 are shown for each day of autoshaping.)

To provide statistical analysis of the findings an F test across sessions was first carried out separately for each group, using Rodger's (1975) method, with Type I error rate set at $E_\alpha = .05$. Second, an overall analysis of variance was carried out between groups collapsed across sessions. Finally, post hoc tests were carried out on the latter group means using Rodger's (1974) method. Mutually orthogonal sets of contrasts of the form $\Sigma c_j m_j = 0$ were sought. Such contrasts are comparisons of weighted combinations of the means in which the coefficient c_j is the weight given to the jth mean m_j. The critical value for rejecting a null contrast was taken from Rodger's (1975) tables of $F_{(E_\alpha)}; \nu_1, \nu_2$. The $F_{(.05)}; \nu_1, \nu_2$ used in this article ensured that the proportion of null contrasts rejected in error would be .05 when all null contrasts are true. For a sample size of 12, the null contrast rejection rate would be $E_\beta = .95$ if any pair of orthogonal contrasts each differ from zero by $\sigma \sqrt{\Sigma c_j^2}$. In this sense, the "power" of the test is quite good against a "reasonable" alternative to the null.

In the within-groups analysis, the negative group showed an increase in number of key peck responses per trial across sessions, $F(2, 66) = 7.59$. The novel group failed to show a significant change in responding across sessions, $F(2, 66) = 1.67$, as did the positive group, $F(2, 66) = .85$.

The outcome of major interest was the difference among the mean number of key peck responses per trial of the three groups (collapsed across sessions). The overall analysis indicated that there were reliable differences among the group means, $F(2, 33) = 3.63$. In the post hoc tests, the set of contrasts chosen showed that the mean number of key peck responses per trial of the positive group was not different from that of the novel group $F(2, 33) = .19$, and that the mean number of key peck responses per trial of the negative group was greater than that of the positive and novel groups combined, $F(2, 33) = 3.44$.

TABLE 1

MEAN NUMBER OF AUTOSHAPING TRIALS PRECEDING THE FIRST TRIAL ON WHICH A KEY PECK OCCURRED: EXPERIMENT 1

Group	Trials to first response
Negative	16
Novel	15
Positive	44

An alternative measure of conditioning is the number of autoshaping trials to the first key peck. The results with the measure are displayed in Table 1. An F test was carried out on the mean trials to first peck of the three groups, with Type I error rate set at $E_\alpha = .05$. The analysis revealed the presence of differences among the group means, $F(2, 33) = 3.92$. Post hoc tests were then carried out on the three means. The set of contrasts chosen showed that the mean trials to first peck of the negative group was not reliably different from that of the novel group $F(2, 33) = .00)$, and that the mean trials to first peck of the positive group was reliably greater than that of the negative and novel groups combined, $F(2, 33) = 3.92$.

Three of the 36 animals in Experiment 1 pecked the lighted key during the first autoshaping trial. Two of these birds were in the positive group and the third was in the negative group. Since these responses occurred before the first pairing of the keylight CS with food, they cannot be regarded as the result of associative learning. Most probably these pecks reflect the general tendency of pigeons to peck at small, bright objects. The important thing to note is that first-trial pecks are a potentially serious source of contamination in trials to first peck data, and, as a source of error, they will tend to decrease the likelihood of finding true differences among group means.

Discussion

The important outcomes of Experiment 1 are (a) the mean number of key peck responses per trial of the negative group was greater than that of the positive group, (b) the negative group showed an increase in number of key peck responses per trial across sessions, whereas the positive group did not show evidence of learning across sessions, and (c) the mean number of autoshaping trials to the first peck was smaller for the negative group than for the positive group. The hypothesis that the reinforcing effectiveness of food increases with its surprise value is well supported by this pattern of results.

The novel group was identical to the positive group using the number of key peck responses per trial measure and identical to the negative group using the trials to first response measures. Hence it is hazardous to draw conclusions concerning the relative effectiveness of reinforcement in this group in Experiment 1.

EXPERIMENT 2

It was decided that the procedure used in Experiment 1 would be improved by the addition of a control group to examine the possible proactive effects of response-independent food delivery (in the discrimination-pretraining procedure) on subsequent autoshaping. Experiment 2 was run with the inclusion of a fourth group (magazine training), which was autoshaped directly after being magazine trained. Aside from the addition of the magazine training group, the only other difference between Experiments 1 and 2 was a reduction in the number of pretraining sessions from seven to four.

Method

Subjects

The subjects were 49 experimentally naive White King pigeons, maintained at 80% of their free-feeding weights. One bird was dropped from the study because of failure to meet a pretraining criterion.

Apparatus

All subjects were run in the chamber used in Experiment 1.

Procedure

Magazine training. The magazine training procedure was the same as it had been in Experiment 1. In order to make the magazine training group as similar to the other groups as possible, the magazine training group was also magazine trained in a colored houselight on the third day. The houselight was blue for four of the birds, green for four, and red for the remaining four.

Pretraining. This phase was skipped by the magazine training group, which entered the autoshaping phase the day after they finished magazine training. For the negative, novel, and positive groups, the procedure was the same as it had been in Experiment 1, except that the animals were given four sessions of pretraining rather than seven. All birds were given exactly 72 food presentations in the pretraining phase (4 × 18). A bird was dropped from the experiment if it ate during fewer than 60 presentations. One bird was dropped after pretraining for failure to meet this criterion. The median total of food presentations during which the animal ate for the 48 birds that completed the experiment was 72 (range = 63–72).

Training (autoshaping). For all four groups, the autoshaping procedure was exactly as it had been in Experiment 1. In Experiment 2, the birds also received a median of 32 autoshaping trials per session (range = 31–34).

The magazine training birds were autoshaped in a houselight color different from the one in which they had received the third day of magazine training. The counterbalancing of houselight colors yielded a total of six magazine training subgroups: magazine trained in R/autoshaped in G, and similarly, R/B, G/R, G/B, B/R, and B/G. Two birds were run in each of these six subgroups.

Results

The mean number of key peck responses per trial of each group on each day of autoshaping are shown in Figure 2. A comparison of the ordinate scales of Figures 1 and 2 shows that the overall rate of responding was much higher in Experiment 2 than in Experiment 1. This may be due to the fact that different strains of pigeons were used in the two experiments; in other experiments by the first author, King pigeons, both White and Silver, have tended to peck at higher rates than Carneaux.

In view of the findings of Experiment 1, t tests were carried out comparing the mean number of key peck responses per trial (collapsed across sessions) of the negative versus positive groups ($t = 1.95$, $p = .057$, two-tailed), the negative versus novel groups ($t = .17$, $p = .83$, two-tailed), and the novel versus magazine training groups ($t = .02$, $p = .98$, two-tailed). These results add confirmation to the finding of Experiment 1 that the mean number of key peck responses per trial of the negative group is greater than the mean of the positive group.

As in Experiment 1, an F test was carried

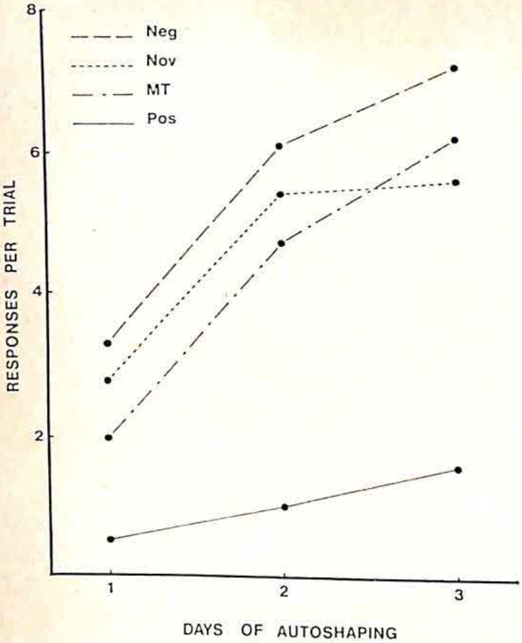

FIGURE 2. Key peck responses per autoshaping trial. (The mean scores of the four groups of Experiment 2 are shown for each day of autoshaping.)

out on the three session means of each group. Three of the groups showed a significant increase in number of key peck responses per trial over the 3 days of autoshaping training: negative, $F(2, 88) = 7.17$, novel, $F(2, 88) = 4.32$, and magazine training, $F(2, 88) = 8.37$. In contrast, the positive group showed no reliable increase in number of key peck responses per trial over sessions $F(2, 88) = .53$.

In Experiment 2, 11 of the 48 birds pecked the key during the first autoshaping trial. This large increase in proportion from Experiment 1 may, like the difference in response rates mentioned above, reflect a strain difference between King pigeons and Carneaux. Four of the birds were in the positive group, four in the negative, two in the novel, and one in the magazine training group. As might be expected with this amount of contamination in the data, the mean differences among groups (shown in Table 2) were small and an F test carried out on the mean trials to first peck of the four groups showed no reliable differences, $F(3, 44) = .30$.

Discussion

The results of Experiment 2 suggest that the delivery of response-independent food in discrimination pretraining had an effect only on the positive group. The mean number of key peck responses per trial of the magazine training, negative, and novel groups did not differ from each other, whereas the means of these three groups were higher than the mean of the positive group. The magazine training, negative, and novel groups all showed a reliable increase in the number of key peck responses per trial over sessions; the positive group did not show evidence of learning over sessions. In Experiment 2 there were no reliable differences in mean trials to first response among the four groups. A possible explanation of this outcome in terms of initial pecking tendencies has already been given.

GENERAL DISCUSSION

Two sets of findings from other laboratories, involving the free delivery of unsignaled reinforcements in advance of autoshaping, are relevant to our interpretation of the results in terms of a surprise hypothesis. First, it can be argued that after pretraining S+ controlled response patterns that competed with autoshaping. In Experiment 6 of his doctoral dissertation, Wasserman (1972) gave naive pigeons 5, 10, or no pretraining sessions in which unsignaled response-independent food was presented on a VT schedule. Birds that received such sessions pecked the key much

TABLE 2

MEAN NUMBER OF AUTOSHAPING TRIALS PRECEDING THE FIRST TRIAL ON WHICH A KEY PECK OCCURRED: EXPERIMENT 2

Group	Trials to first response
Negative	12
Novel	22
Positive	19
Magazine training	19

less in autoshaping than the birds not pretrained in this manner. Wasserman interpreted this finding in terms of adventitious reinforcement and response competition. In pretraining, the unsignaled presentation of food was presumed to instrumentally reinforce various behaviors, such as shuttling in front of the intelligence panel, which may have interfered with key pecking during the autoshaping sessions.

If we assume that such reinforced responses came under the control of S+ in our pretraining procedure, such behavior would compete with autoshaping in the positive group but not in the Negative group, since it would have extinguished, if it occurred at all, in the presence of S−. Thus, it could be argued that the difference observed in our study between the autoshaping in these groups was not due to differential formation of the CS–US association, but was due to differential interference from competing behaviors.

Further research might provide information useful in deciding between the surprise hypothesis and the response competition interpretations of the present findings. One major difficulty in interpreting the present data arises because our experimental manipulation confounded the conditions of stimulation (S+, S−, or S^N) under which the key peck was *acquired* and the conditions of stimulation (S+, S−, or S^N) under which responding was *measured*. Thus it is presently impossible to separate acquisition effects from other influences on performance. A possible way around this difficulty would be to train animals to key peck in S+ or S− (as was done in the present studies) and then in a subsequent phase to test both groups in S^N. If the positive group responds less than the negative because of competing responses controlled by S+ (or because of any other influences on performance exerted by S+ or S−), then the difference in response rates between the two groups should disappear when they are commonly tested in S^N. If, as required by the surprise hypothesis, S+ and S− differentially affect the acquisition of conditioned responding, the difference between the two groups should still be seen when they are tested in S^N.

Rather than accounting for our results in terms of a response competition hypothesis, one can just as easily account for Wasserman's (1972) finding in terms of the surprise hypothesis. His two pretrained groups received either 5 or 10 occasions on which to learn the predictive relationship between the experimental chamber and the arrival of food. Since the delivery of food was presumably less surprising in the chamber to the pretrained pigeons than to the group that received no such sessions, food may have been a less effective reinforcer for those groups such that autoshaping was retarded.

A second finding of relevance to our interpretation was published by Engberg, Hansen, Welker, and Thomas (1972), who conducted an autoshaping experiment essentially identical to Wasserman's Experiment 6. They obtained similar results and interpreted them in terms of "learned laziness." According to this account, in the pretraining phase with free food the animals learned that their behavior was irrelevant to the delivery of food. This experience presumably induced a disposition not to respond, which generalized to the autoshaping situation. Engberg et al.'s viewpoint and ours may be compared as follows: The surprise hypothesis attributes the US-only pretraining effect to prior CS–US learning; the learned laziness hypothesis attributes it to prior response–US learning.

Can the learned laziness hypothesis be reconciled with our data? The flavor of that hypothesis suggests that learned laziness should be a general phenomenon of the animal's behavior and should be independent of the particular situation in which pretraining occurred. If so, we would expect that all groups in Experiment 2 should autoshape at lower rates than the magazine training group. However, this was the case only for the positive group. On the other hand, it could be argued that learned laziness is stimulus bound and that we should expect it to be manifested only in that condition in which free reinforcements were delivered, namely S+. Then the hypothesis becomes essentially indistinguishable from the response interference hypothesis. Furthermore, such an assumption would deprive the learned laziness hypothesis of the characteristic of being applicable across various situations. It would then forfeit its claim to be the appetitive analogue of the phenomenon identified by Seligman and Maier (1967) as learned helplessness.

In summary, the learned laziness hypothesis is not easily reconciled with the data of the present study. The surprise hypothesis, on the other hand, not only predicted the outcomes of the present two experiments but also provides a consistent interpretation of the US-only pretraining effect. In view of the wider applicability of the surprise hypothesis, an account of the US-only pretraining effect in terms of prior CS–US learning appears to be preferable to one in terms of prior response–US learning.

Finally, the present data may also be related to previous data reported by Hall and Honig (1974), who used a procedure similar to the present one. The relevant data come from two groups of pigeons pretrained with free food in advance of autoshaping. One group (true discrimination) was treated almost identically to our novel group in Experiment 1: 7 days of pretraining with free food in a houselight discrimination procedure (green = S+, red = S−) followed by autoshaping in a white houselight. For the second group (pseudodiscrimination) the operation of the VT 60-sec and extinction food schedules was uncorrelated with the houselight color. After 7 days of pseudodiscrimination training, the pseudodiscrimination group was also autoshaped in the white houselight.

Hall and Honig found that the true discrimination group autoshaped at higher rates than the pseudodiscrimination group. They interpreted their results in terms of generalized transfer of training: Animals presented with a correlation between environmental events and a US (the true discrimination group) may learn a new correlation (key-light–US) more rapidly than animals given uncorrelated training (the pseudodiscrimination group). However, these data can also be interpreted in accordance with the surprise hypothesis; true discrimination as opposed to pseudodiscrimination training should "neutralize" contextual cues that would otherwise signal the US and act like S+ in our studies to depress autoshaping.

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Self-Control and General Models of Choice

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In a choice between an immediate, small reward and a delayed, large reward, an organism may be said to exhibit "self-control" if it chooses the large reward. Given an opportunity in advance to avoid this choice, pigeons may commit themselves to the large reward, but if the choice is encountered they will almost always select the immediate, small reward. This preference reversal as a function of temporal distance from reinforcement is compatible with some general theories of choice. The present study tested one such model, developed by Rachlin and his co-workers, which assumes that (a) relative choice responses "match" the relative values of the outcomes; and (b) outcome values equal the product of rate and amount of reinforcement. Pigeons chose between 4.5 and 1.5 sec access to grain with equal or unequal delays of reinforcement. When delays consisted of unequal fixed-interval schedules the shorter delay terminated with the smaller reward, and preference for the large reward tended to increase with the absolute size of the delays (consistent with the matching model). However, preference for the large reward increased with delay size even when the delays were equal, a violation of the model. It is shown that both findings are derivable from either models of choice (developed by Logan and by Rachlin and Ainslie) based on gradients of reinforcing value, or from a more general model, previously applied to other choice situations, which assumes preference to depend on the reduction in time to reinforcement correlated with the alternatives.

The choice between an immediate, small reward and a delayed, large reward has long been regarded as a significant one. Selection of the immediate, small reward has frequently been taken as a sign of "impulsiveness" (e.g., Ainslie, 1974, 1975), while selection of the delayed large reward has been taken as evidence of "self-control" (e.g., Rachlin, 1974). Pigeons under a variety of conditions select the immediate, small reward (Ainslie, 1974), although with extended training they may come to select the delayed, large reward (Fantino, 1966). If, in advance of the choice, a pigeon is given the opportunity to avoid the choice and commit itself to receiving the larger reward, it may do so. The further in advance this opportunity is presented, the more likely it is that the organism will make the commitment to the larger reward (Ainslie, 1974; Rachlin & Green, 1972).

Ideally, such relationships could be understood within the framework of a single quantitative model of choice. Several functions describing the relation of reward effectiveness to delay of reinforcement are relevant to the problem (Ainslie, 1975). Some functions, including those of Hull (1943) and Chung (1965), are exponential, while others, including those of Logan (1965), Rachlin and Green (1972) and Fantino (1969) are functions which are more upwardly concave than exponential functions, that is, functions for which an increase in delay reduces reward effectiveness more sharply than in the exponential case. Ainslie (1975) has shown that exponential functions with equal time constants cannot account for the reversal of preference reported by Rachlin and Green (1972), and that only more upwardly concave functions are appropriate. Discussion of two of the functions—those proposed by Logan (1965)

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and Fantino (1969)—will be deferred until the end of this article as the present study is more directly a test of the Rachlin and Green (1972) model.

Rachlin and Green (1972) suggested that much of the data on self-control may be derivable from a general model proposed by Baum and Rachlin (1969):

$$V_1/V_2 = A_1/A_2 \times D_2/D_1 = R_1/R_2, \quad (1)$$

where V represents the value of an alternative, A the amount of reinforcement, D the delay of reinforcement, and R the number of choice responses for the alternative. (Similar versions have been formulated by Chung & Herrnstein, 1967; Herrnstein, 1970; and Premack, 1965.) Suppose, for example, that A_1 represented 4 sec of reinforcement and A_2 2 sec of reinforcement. If A_1 were delivered after a delay of 10 sec and A_2 were delivered immediately (delay $= 0$ sec), then $V_1/V_2 = 0.00$, and the organism should select the smaller reward, A_2. Now suppose that 20 sec were added to the delays associated with A_1 and A_2, so that A_1 was delivered after 30 sec and A_2 after 20 sec. Equation 1 requires a reversal of preferences. The organism should select the larger reward, A_1, since $V_1/V_2 = 1.33$ (i.e., $R_1 > R_2$). These predictions are consistent with the results of the studies of commitment mentioned earlier (Ainslie, 1974; Rachlin & Green, 1972). When the organism was close in time to A_1 and A_2, it chose the smaller reward, A_2. When the organism was distant in time from A_1 and A_2 but a constant difference in delay was maintained between them, the organism made a response appropriate to receiving the larger reward, A_1. Equation 1 thus appears to provide a basis for understanding the reversal phenomenon in studies of self-control.

Most of the support for Equation 1 has come from studies of choice in which amount of reinforcement or delay of reinforcement has been manipulated with the other parameter held constant (e.g., Brownstein, 1971; Catania, 1963; Chung & Herrnstein, 1967). Relatively few studies have been performed in which the alternatives have differed both in amount and delay of reinforcement (Logan, 1965; Logan & Spanier, 1970; Navarick & Fantino, 1975; Rachlin & Green, 1972; Ten Eyck, 1970). Only two of these studies have examined the reversal phenomenon (Logan & Spanier, 1970; Rachlin & Green, 1972), and both had features which could complicate the interpretation of Equation 1. For example, the Logan and Spanier study, with rats, defined amount of reinforcement in terms of number of food pellets rather than duration of access to the reinforcer, the customary parameter in tests of Equation 1. The Rachlin and Green study was explicitly designed to investigate commitment, not to test Equation 1, and thus included relatively complex contingencies. The first part of our study employed a more conventional choice procedure in order to test the adequacy of Equation 1 under conditions which should have produced a reversal of preferences.

The procedure used to study choice was the concurrent-chains procedure (Autor, 1969; Navarick & Fantino, 1972a, 1972b). In a chained schedule (Ferster & Skinner, 1957), the organism receives reinforcement after completing two or more schedules in succession. Each schedule is associated with a different stimulus, so that completion of a particular schedule (other than the one terminating in reinforcement) produces the stimulus for the next schedule. In the concurrent-chains procedure, the organism responds on two keys illuminated by the color associated with the initial links of two chained schedules. Occasionally, and unpredictably, a response changes the color on the side where the response was made and causes the other key to become dark and inoperative. Further responding on the lighted key produces reinforcement according to the schedule associated with the terminal-link stimulus. Following reinforcement, the keys are again lighted by the initial-link stimuli and another cycle beings. A choice proportion for the terminal-link schedule is calculated by dividing the number of *initial-link* (choice) responses on the appropriate side by the total number of initial-link responses

$$\left(\frac{R_1}{R_1 + R_2}\right).$$

In Experiment 1, pigeons chose between

two unequal reinforcement durations, 4.5 sec (A_1) and 1.5 sec (A_2). The delay of reinforcement associated with A_2 was always 10 sec shorter than the delay associated with A_1. With relatively short delays ($D_1 = 12$ sec, $D_2 = 2$ sec), Equation 1 requires preference for A_2 (since $R_1/R_2 = .5$); that is, the pigeons should exhibit "impulsiveness." As D_1 and D_2 are increased by a constant amount, Equation 1 describes a consistent increase in the choice proportion for A_1 and eventually a substantial preference for A_1 over A_2 ("self-control"). These predictions were tested both when the delays were programmed on fixed-interval (FI) schedules and on "blackout" (response-independent) schedules. A second prediction of Equation 1 was tested in Experiment 2: If the delays of reinforcement associated with A_1 and A_2 are equal, preference for the larger reward should remain constant with variations in the size of the delays (since R_1/R_2 would then simply equal A_1/A_2). Violation of either prediction would mean that Equation 1 does not provide an adequate general model of choice nor an adequate account of preference reversals in studies of self-control. Interestingly, the formulations of Logan (1965) and Fantino (1969) make similar predictions when the delays are unequal, but Fantino's formulation (unlike either Logan's or Baum & Rachlin's, 1969) requires *increasing* preference for the larger reward as the absolute size of equal delays is increased. Thus, the outcome of the second test helps assess the viability of both Equation 1 and Logan's equation as opposed to Fantino's equation.

GENERAL METHOD

Subjects

Ten male White Carneaux pigeons and two Silver King pigeons (SK-1 and SK-2) were maintained at approximately 80% of their free-feeding weights. The Silver King pigeons had previously served in an experiment on autoshaping and automaintenance (Hursh, Navarick, & Fantino, 1974); the remaining pigeons were experimentally naive. Six pigeons served in Experiment 1 and six in Experiment 2.

Apparatus

A standard experimental chamber (Ferster & Skinner, 1957) contained a solenoid-operated grain hopper centrally located between two circular translucent response keys. The keys were mounted 3 in. (7.62 cm) apart and 9 in. (22.9 cm) above the floor and were illuminated from behind by white, red, or green Christmas lights. Two 6-W houselights provided general illumination. During reinforcement, the houselights were turned off and the feeding aperture was lit; the keylights remained on in appropriate experimental conditions (see Procedure section). Each peck on a lighted key produced a feedback click by operating a 110-V ac relay behind the intelligence panel. A force of approximately 12 g (.118 N) was required to operate the keys. White noise masked extraneous sounds. Standard electromechanical equipment was located in an adjacent room.

Procedure

Concurrent-chains schedule. Figure 1 presents a diagram of the concurrent-chains procedure. Access to the terminal links was made available on two equal variable interval (VI) 1-min. schedules. When the terminal link on one side became available, the VI timer associated with that side stopped while the alternate timer continued to operate. Entry into a terminal link stopped the alternate VI timer and produced

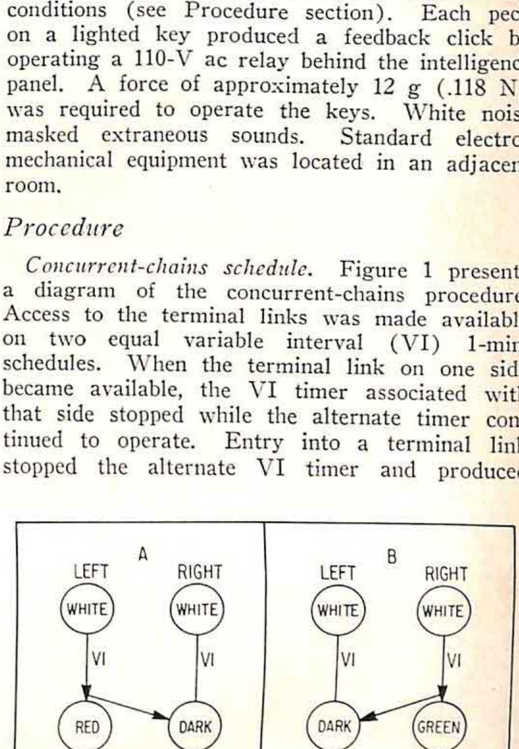

FIGURE 1. Diagram of the concurrent-chains procedure, Experiment 1. (The left portion of the figure illustrates the sequence of events when reinforcement was received on the left side; the right portion of the figure, the sequence of events when reinforcement was received on the right. When fixed-interval schedules were programmed in the terminal links, the key colors became red or green upon entry into the left or right terminal links, respectively. When blackout schedules were programmed, both keylights and the houselight were turned off.)

stimulus conditions appropriate to the terminal-link schedule. If FI schedules were programmed in the terminal links, entry into the terminal link changed the key color on the side pecked (from white to red on the left; from white to green on the right) and caused the other key to become dark and inoperative (no feedback clicks). The houselights remained on. Further responding on the lighted key produced reinforcement according to the requirements of the FI schedule. If blackout conditions were programmed in the terminal links, entry into the terminal link turned off both the houselights and the keylights. At the end of the prescribed interval, reinforcement was delivered independently of responding. Following reinforcement under either the FI or blackout schedule, the initial-link (white) stimuli were reinstated and another cycle began. Both VI timers restarted at this time except in the following case: If both VI timers had arranged access to their respective terminal links at the time one of the terminal links was entered, the terminal link not entered remained available (and the associated VI timer remained stopped) upon return to the initial links.

A pair of terminal-link schedules was presented for choice until the daily choice proportions satisfied the following stability criterion: After 18 days (and every day thereafter until stability was achieved), the choice proportions for the last 9 sessions were divided into three successive blocks of 3 sessions. Preferences were considered stable when (a) the means of these blocks did not differ from each other by more than .05; and (b) there was neither an upward ($M_1 < M_2 < M_3$) nor a downward ($M_1 > M_2 > M_3$) trend in the block means. The reported values are averages over this 9-day period. Sessions were conducted 7 days per wk.

EXPERIMENT 1

Unequal Delays of Reinforcement

In this experiment—involving 6 of the 12 subjects—the delays of reinforcement were unequal and the absolute size of the delays was varied. The delay of reinforcement associated with the 1.5-sec hopper duration (D_2) was 10 sec shorter than the delay associated with the 4.5-sec hopper duration (D_1). Four pairs of values were studied: 12 sec vs. 2 sec, 20 sec vs. 10 sec, 30 sec vs. 20 sec, and 40 sec vs. 30 sec. For these amounts and delays of reinforcement, Equation 1 requires choice proportions for the larger hopper duration of .33, .60, .67, and .69, respectively. In other words, a reversal of preferences should have occurred (since .50 = indifference). For three of the six birds, the delays of reinforcement were programmed on FI schedules, while for the other three birds the delays were programmed on blackout schedules. Equation 1 requires equivalent results for these schedules.

Method

Following two preliminary conditions (explained later), all birds received delays of 12 sec vs. 2 sec. The remaining three pairs of delays (see preceding paragraph), were assigned in different orders. After the four pairs of delays were presented, they were replicated in a new sequence. The longer hopper duration and longer reinforcement delay were always presented on the same side for a given bird.

During the replication phase of the study, the initial links were modified for birds receiving blackouts in the terminal links. In a previous concurrent-chains study using blackouts (Chung & Herrnstein, 1967) switching responses during the initial links were penalized with a changeover delay (COD). A peck during the initial links could not produce a terminal link if the preceding response was on the other key. Subsequent responses on the same key also could not produce a terminal link for a period of 1 sec (timed from the changeover response). The same COD procedure was employed in the present replication of the blackout conditions. It should be noted that CODs are rarely used in concurrent-chains studies, although they are common in studies employing regular concurrent schedules (e.g., Herrnstein, 1961, 1964).

Preliminary Conditions

There were two preliminary conditions. In the first, the pigeons chose between equal hopper durations (1.5 sec) and equal delays of reinforcement (2 sec). Choice proportions which deviated from .50 by more than .05 were assumed to indicate possible key bias, necessitating a modification of the predictions of Equation 1. One pigeon's (1047) choice proportion for one side was .70, while the remaning birds gave values which deviated from .50 by .05 or less. The manner in which Equation 1 was modified for Pigeon 1047 is described in the Results and Discussion section.

The second preliminary condition ascertained whether the pigeons could discriminate a difference between reinforcement delays of 10 sec. The reinforcement delay was increased to 12 sec on whichever key gave the higher choice proportion in the first condition, while the reinforcement delay on the other key remained at 2 sec. All pigeons showed an appropriate increase in preference for the key associated with the shorter delay.

Finally, in the third preliminary condition—which was also the first experimental condition—the hopper duration was increased to 4.5 sec on the key associated with the longer delay of reinforcement, while the hopper duration was held constant at 1.5 sec on the other key. In all cases, there was an appropriate increase in preference for the key associated with 4.5-sec hopper duration (and 12-sec reinforcement delay). Thus, the first three conditions in this phase of the study established that all six pigeons discriminated between a 10-sec difference in reinforcement delays and a 3-sec difference in hopper durations.

Results and Discussion

Figure 2 illustrates the major findings of Experiment 1. Preferences for the larger reward are shown as a function of the four sets of terminal-link intervals—2 sec vs. 12 sec, 10 sec vs. 20 sec, 20 sec vs. 30 sec, and 30 sec vs. 40 sec. The panels on the right illustrate the effects of FI schedules in the terminal links, while the panels on the left show the effects of the blackout schedules. Pigeon 1047 exhibited a substantial key bias in Condition 1, necessitating a change in the predictions from Equation 1. Baum and Rachlin (1969) have suggested that key bias combines multiplicatively with schedule parameters. Since a choice proportion of .70 was obtained on the side subsequently delivering the longer hopper duration (A_1), the ratio V_1/V_2 was multiplied by a factor of 70/30.

None of the three pigeons on blackout schedules showed functions resembling the predicted ones. In the first determination (without a COD), the pigeons were essentially indifferent to alternatives with all pairs of terminal-link intervals. In the replication (with a 1-sec COD), two of the three birds (SK-1 and 1781) showed substantial increases in preference for the larger reward as the terminal-link intervals were increased from 2 sec vs. 12 sec to 10 sec vs. 20 sec, but these increases were much smaller than those predicted by Equation 1. Moreover, all three pigeons showed indifference (choice proportions of about .50) across the three largest pairs of terminal-link intervals (10 sec vs. 20 sec to 30 sec vs. 40 sec) in marked contrast to the strong preferences for the larger reward required by Equation 1.

The theoretical functions were more closely approximated with FI schedules in the terminal links. In the first determination (no COD), two of the three brids (SK-2, Y-20) showed reversals of preference—first preferring the smaller reward and then the larger—as the terminal-link intervals were increased from 2 sec vs. 12 sec to 10 sec vs. 20 sec, but preferences with larger intervals tended to deviate substantially from predicted values. The third pigeon (1047), one with a large key bias, showed very little change in preference as a function of terminal-link intervals. In the second determination (also without a COD), two birds (Y-20 and 1047) gave monotonically increasing functions, consistent with Equation 1, but the third bird (SK-2) now showed little change in preference across terminal-link intervals (although preference for the larger reward was higher with intervals of 30 sec vs. 40 sec than with shorter intervals). A Friedman two-way analysis of variance by ranks was applied to choice proportions averaged over the two determinations. The test revealed a significant increase in choice proportions as a function of the size of the terminal links ($\chi_r^2 = 8.2$, $N = 3$, $K = 4$, $p = .017$). Thus, FI schedules yielded closer approximations to predicted preferences than did blackouts, but in neither case was Equation 1 consistently supported. The difference in effects of blackout and FI schedules probably arises from the fact that blackouts (unlike FIs) fail to provide different stimulus consequences for the two choice responses. On the blackout schedules, the immediate consequence of entering a terminal link was darkness, and darkness mediated both delays. On the FIs, entry into the terminal links produced different key colors that remained until the moment of reinforcement. Increases in preference are apparently most likely to occur when the different consequences of the choice responses are accentuated.

EXPERIMENT 2

Equal Delays of Reinforcement

The results of Experiment 1 gave partial support to Equation 1. With unequal FI schedules in the terminal links, two of three subjects showed monotonically increasing

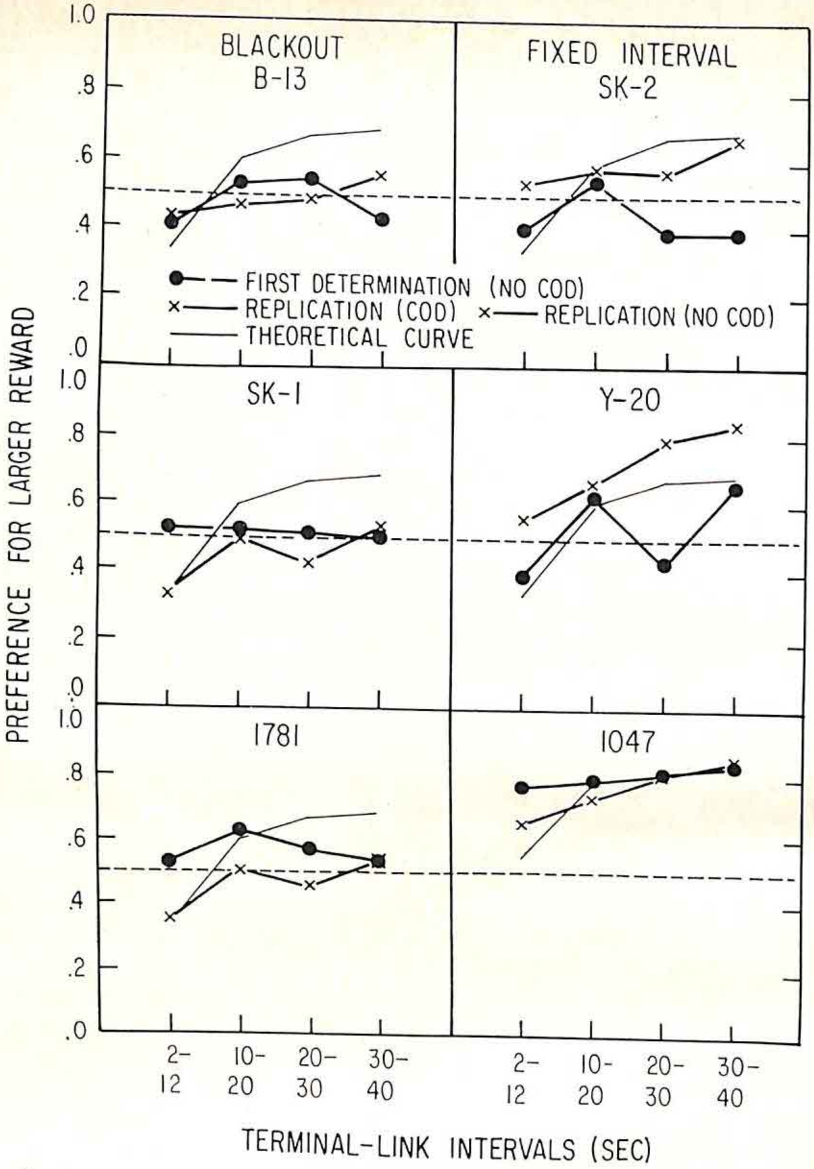

FIGURE 2. Preference for the larger reward, 4.5-sec access to grain, as a function of terminal-link intervals, Experiment 1. (The shorter interval was always associated with the smaller reward, 1.5-sec access to grain. The left portion of the figure shows results with blackouts in the terminal links, the right portion, the results with fixed-interval schedules in the terminal links. The thin line in each panel represents choice proportions predicted by Equation 1, the heavier lines, the obtained values. Functions connected by dots are original determinations, functions connected by crosses, replicated values.)

preferences for the larger reward as the intervals were increased. In Experiment 2, the pigeons chose between hopper durations of 4.5 and 1.5 sec with equal terminal-link intervals. In contrast to the case of unequal terminal-link intervals, Equation 1 now requires a constant preference of .75 for the larger reward with increases in the duration of the terminal links. Any systematic change in preference would make Equation 1 untenable as a general model of choice and, therefore, untenable as an explanation for preference reversals in studies of self-control.

Method

The six remaining pigeons served in an ABA design in which Condition A was a relatively short delay of reinforcement (e.g., 5 sec) and Condition B was a relatively long delay (e.g., 20 sec). Since the precise schedule values varied across birds, they will be fully described when the results are presented.

As in Experiment 1, half of the subjects received FI schedules in the terminal links and half received blackouts. Also, preliminary sessions were employed to estimate key bias and test the pigeons' sensitivity to the difference between hopper durations. In the first condition, the hopper durations (1.5 sec) and the reinforcement delays (5 sec) were equal. Substantial deviations of the choice proportions from .50 would indicate key bias. In the second condition, the hopper duration was increased to 4.5 sec on whichever key had the lower choice proportion in the previous condition (to avoid extreme preferences on any favored side). All of the pigeons with FI schedules in the terminal links showed appropriate increases in preference for the side associated with the longer (4.5-sec) hopper duration, but none of the pigeons on blackout schedules showed appreciable change. The blackout condition was therefore modified by turning on the appropriate key color (red or green) during the terminal link, while the houselight remained off. This was expected to make the differential consequences of initial-link pecks on the left and right sides more discriminable, as discussed above. The modified blackout procedure increased preferences for the longer hopper duration only slightly. Finally, regular FI schedules were substituted for the response-independent schedules, and two of the three pigeons showed substantial increases in preference. The third pigeon also showed an appropriate preference for the longer hopper duration after nine training sessions in which the initial schedules were reduced to continuous reinforcement (CRF); that is, every initial-link response achieved entry into the terminal link. Thus, for each of the six pigeons, reinforcement delays were ultimately arranged on FI schedules.

Results and Discussion

With equal terminal-link intervals, a clear and consistent pattern of results emerged directly contradictory to Equation 1. Equation 1 requires that preference for the larger reward be unaffected by varying the duration of the terminal links. In virtually every case, preference increased sharply with increases in the duration of the terminal links. These results are illustrated in Figure 3, for subjects that started the experiment with FI schedules, and in Figure 4, for subjects that started with blackouts and subsequently received FI schedules. Both graphs show

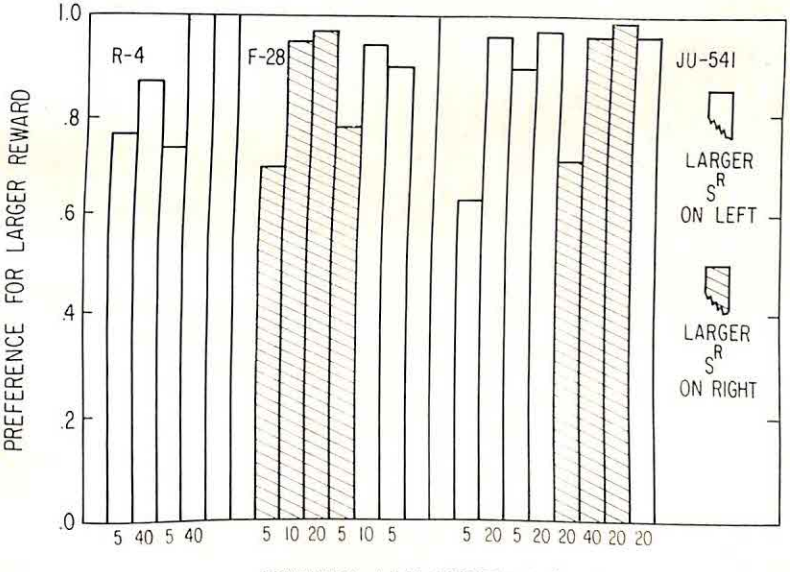

FIGURE 3. Preference for the larger reward with equal terminal-link intervals, Experiment 2. (The intervals were equal for the larger and smaller rewards and were programmed with fixed-interval schedules.)

preferences for the larger reward as a function of the duration of the terminal-link intervals. An increase in the terminal links from 5 to 20 sec was sufficient to produce extremely strong preferences for the larger reward in each of the four subjects (JU-541, F-28, 1006, and 6310) exposed to these conditions. The remaining two birds (R-4 and 624) showed extreme preferences when the terminal links were increased from 5 to 40 sec but were not tested with other values. Thus, while the slope of the functions relating preference to the size of the terminal links probably varies somewhat across subjects (e.g., an increase to 10 sec was sufficient to increase preference for Bird F-28 but was insufficient for 6310), the choice proportions apparently reach 1.00 with intervals of 40 sec. All subjects showed decreases in preferences when the terminal links were subsequently reduced, indicating that the effect was reversible. These results are a direct violation of Equation 1 and have important implications for other choice models that could potentially account for preference reversals in studies of self-control.

GENERAL DISCUSSION

Demands upon an organism's self-control arise when a choice must be made between an immediate, small reward and a delayed, large reward. Ainslie (1974) and Rachlin and Green (1972) have shown that organisms may achieve self-control by committing themselves in advance to the larger reward and thereby avoiding the choice. This strategy of commitment necessarily involves a reversal of preferences over time (Ainslie, 1975; Rachlin, 1974). A commitment in advance becomes necessary because an organism habitually chooses the smaller reward when it is immediately available. A commitment in advance is possible only if the larger reward is preferred to the smaller when the commitment is made.

A reversal of preference is one implication

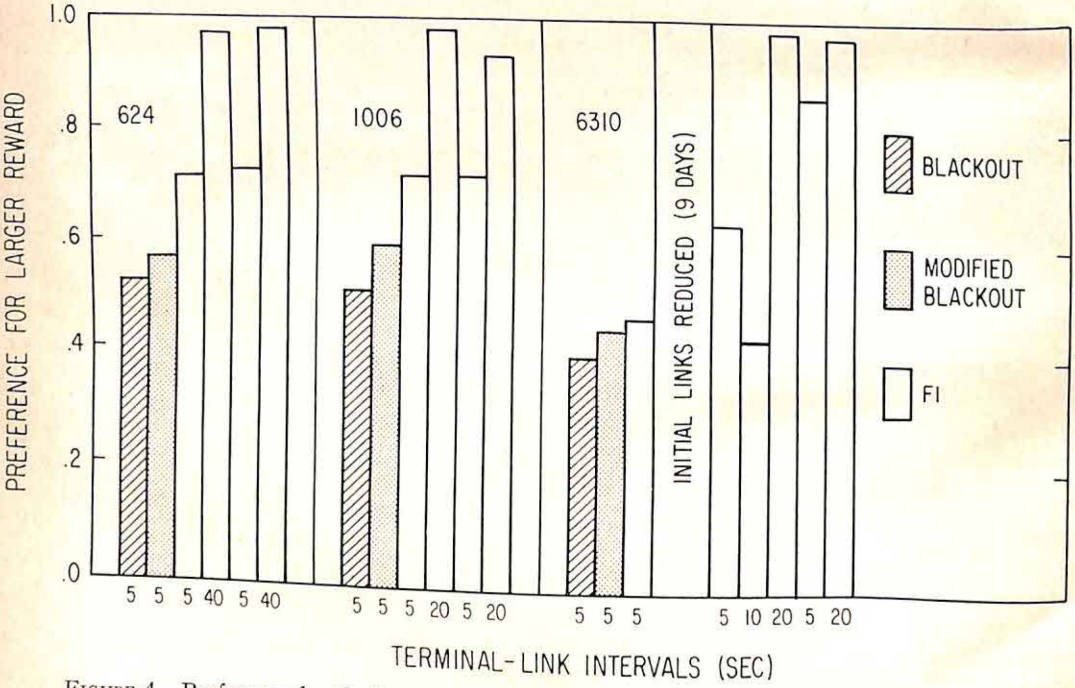

FIGURE 4. Preference for the larger reward with equal terminal-link intervals, Experiment 2. (The pigeons initially received blackouts in the terminal links but subsequently received fixed-interval schedules. In the modified blackout procedure red or green keylights were turned on upon entry into a terminal link, while the houselights were turned off. Reinforcement was delivered independently of responding, as in the regular blackout procedure.)

of the choice model, Equation 1, proposed by Baum and Rachlin (1969), and similar models proposed by Premack (1965) and Herrnstein (1970). When a large reward is associated with a long delay of reinforcement and a small reward is associated with a short delay of reinforcement, these models predict a monotonic increase in preference for the large reward with increases in delay (the difference between delays remaining constant). However, when FI schedules are used to arrange the delays, one might expect the predictions of Equation 1 to be imprecise. Duncan and Fantino (1970) and Killeen (1970) examined choice for unequal FI schedules terminating with *equal* reinforcement durations. They found higher choice proportions for the shorter FIs than those required by Equation 1 (which describes matching of choice proportions to relative rates of reinforcement). Interestingly, the present data supported Equation 1 when unequal FI schedules were used; preference for the large reward increased with delay. It was when equal FI schedules were used that Equation 1 was clearly violated; in this case, preference for the large reward also increased with delay. An adequate general model of choice apparently must describe increases in preference whether the delays are equal or unequal.

Ainslie (1975) and Rachlin (1970, pp. 185–189) have proposed such a model based on gradients of reinforcing value. While Ainslie and Rachlin applied the model explicitly only to the case of unequal delays of reinforcement, the model is also applicable to the case of equal delays. The top part of Figure 5 illustrates the manner in which Rachlin (1970) applied the model to the former case; the lower half of the figure illustrates how the model may be applied to the latter case. The larger reward (A_L) and the smaller reward (A_S) are assumed to generate gradients of reinforcing value that decrease with temporal distance from reinforcement. The dotted vertical lines, X and Y, represent choice points at varying temporal distances. In the top panel, point Y represents a choice between an immediate, small reward and a delayed, large reward.

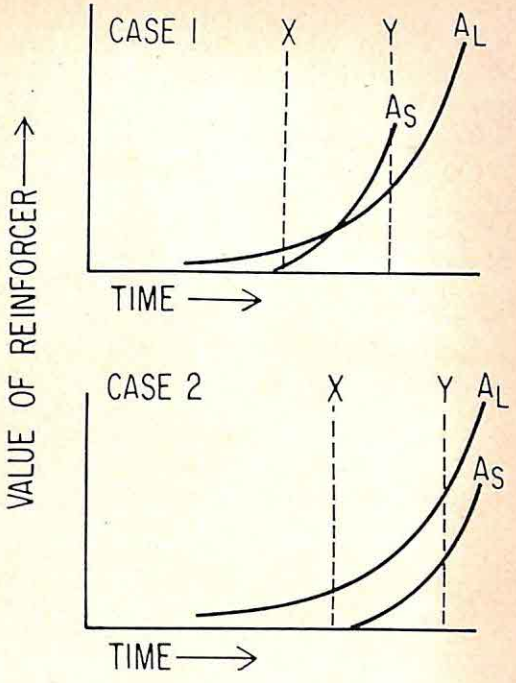

FIGURE 5. The reinforcement-gradient model of self-control proposed by Rachlin (1970). (Case 1 is Rachlin's original formulation for the case in which the small reward, A_S, is associated with a short delay of reinforcement, and the larger reward, A_L, is associated with the longer delay of reinforcement. The functions represent the value of the reinforcer at varying temporal distance from reinforcement, the moment of reinforcement corresponding to the maximum height of the function. The symbols X and Y are choice points. Case 2 is a derivative of Case 1 introduced here as a possible interpretation of results with equal delays of reinforcement.)

In Rachlin's (1970) original formulation, the organism was assumed to choose the reward with the higher reinforcing value at the moment of choice. From the standpoint of a probabilistic choice model (such as Equation 1), it may be assumed that the choice proportion will be proportional to the height of the gradient. Since A_S is higher than A_L at Point Y, the organism should choose A_S. At Choice Point X, the organism is more distant in time from A_L and A_S, but the difference between reinforcement delays is the same. Since the gradient for A_L is higher than that for A_S at Point X, the organism should choose A_L, a reversal of preferences.

In the bottom panel, the organism is equally distant in time from reinforcement with A_L and A_S; this is analogous to having equal terminal-link intervals in the present study. At Choice Point Y, the organism is relatively close to reinforcement and should show a stronger preference for A_L than A_S, since the gradient for A_L is higher. At Point X, the time to reinforcement is substantially greater than at Point Y, and the gradient for A_S drops effectively to zero, while the gradient for A_L remains greater than zero. The result should be an exclusive preference for A_L. The present data were consistent with this expectation. In almost every case, increasing the terminal-link intervals resulted in virtually exclusive preferences for A_L (choice proportions of .95 or more). An intermediate increase occurred in only one case (R-4, Figure 3), and even in this case an exclusive preference developed in the second determination. The Ainslie–Rachlin model appears to provide an excellent description of the present data.

In its present form, the Ainslie–Rachlin model lacks important mathematical details: It does not specify an equation for generating gradients of reinforcing value, nor does it state the decision rule by which the organism compares the values of A_L and A_S at a given point in time. Depending on the nature of these assumptions, the model may, or may not, adequately describe the present data. For example, Logan's (1965) choice model generates gradients similar to those in Figure 5, and it adequately describes reversals of preference with unequal delays of reinforcement. However, because of the decision rule tentatively adopted by Logan, his model fails to describe increases in preference with equal delays of reinforcement. Logan's equation for calculating reinforcing values may be stated as follows:

$$V = K - .13D^{.5}, \qquad (2)$$

where K is a function of amount, D is delay in seconds, and .13 and .5 are empirical constants. Equation 2 gives the values of A_L and A_S at a given moment in time, but it does indicate how the organism compares these values. Logan favored an "additive" rule for comparing values:

$$P_L = .5 + F[(K_L - J_L) - (K_S - J_S)], \qquad (3)$$

where P is the probability of choosing the larger reward (L), $J = .13D^{.5}$ (from Equation 2), and F is a monotonic function. Equation 3 generally states that choice is a function of the *difference* between the values of A_L and A_S, and it describes reversals of preference for A_L and A_S with unequal delays of reinforcement (Ainslie, 1975). However, with delays equal, Equation 3 requires a constant preference for A_L as the duration of the delays increases, since the equation reduces to

$$P_L = .5 + F(K_L - K_S), \qquad (4)$$

and that prediction is inconsistent with the increases in preference observed in Experiment 2. Logan's (1965) equation for generating gradients of reinforcing value (Equation 2) can describe increases in preference when the decision rule is based on the *ratio* of the values of A_L and A_S:

$$P_L = .5 + F(K_L - J_L)/[(K_L - J_L) + (K_S - J_S)]. \qquad (5)$$

The decision rule in Equation 5 is essentially equivalent to that in the Baum–Rachlin model (Equation 1). The models differ in that Equation 5 assumes an "additive" relationship between amount and delay (cf. Spence, 1956) while Equation 1 assumes a "multiplicative" relationship (cf. Hull, 1943). The remaining possibility is a multiplicative relationship between amount and delay and an additive decision rule:

$$P_L = .5 + F[(K_L/J_L) - (K_S/J_S)]. \qquad (6)$$

With equal delays of reinforcement, Equation 6 describes a *decrease* in preference for A_L as the durations of the delays increase. An adequate description of the present data thus requires an additive rule for combining amount and delay of reinforcement and a multiplicative rule for comparing the values of alternatives (as in Equation 5).

Toward A General Model Of Choice

Although Equation 5 describes reversals of preference with unequal delays, and increases in preference with equal delays, it has a serious shortcoming, especially in the context of the concurrent-chains procedure. In this procedure, the duration of either the initial links or the terminal links could be extended to increase the temporal distance from reward. Increasing the duration of the initial links apparently has an effect opposite to that of increasing the duration of the terminal links: Preferences decrease in the former case and increase in the latter. This generalization concerning initial-link duration is based on several previous studies (e.g., Fantino, 1969; Hursh & Fantino, 1974; Wardlaw & Davison, 1974; Frankel & vom Saal, in press) as well as some data collected in this study (Bird 6310, Figure 4). As Equation 5 lacks separate parameters for the initial and terminal links, it must make the same prediction when either parameter is manipulated. A model that incorporates these parameters separately and that adequately describes the present data (as well as data in a variety of other situations) has been proposed by Fantino (1969) and modified by Squires and Fantino (1971). Since both versions make comparable predictions for the values studied in the present experiments, we present only the simpler version:

$$\begin{aligned}\frac{P_{\mathrm{L}}}{P_{\mathrm{L}}+P_{\mathrm{R}}} &= \frac{T-t_{l}}{(T-t_{l})+(T-t_{r})} \\ &\qquad (\text{when } t_{l}<T,\ t_{r}<T) \\ &= 1 \quad (\text{when } t_{l}<T,\ t_{r}>T) \\ &= 0 \quad (\text{when } t_{l}>T,\ t_{r}<T), \qquad (7)\end{aligned}$$

where P_{L} and P_{R} are the number of responses on the left and right, respectively, t_l is the terminal-link interval on the left, t_r is the terminal-link interval on the right, and T is the average time to reinforcement from the onset of the initial links. The value of T depends on the durations of the initial-link and terminal-link schedules. When the initial-link schedules are of equal duration, the average time to reach a terminal link is one half the duration of either schedule alone. As VI 1-min. schedules were employed in the present study, the average time to reach a terminal link would be 30 sec. In the concurrent-chains procedure, each terminal link is sampled about half the time so that the average time spent in the terminal links would be $.5\ (t_l) + .5\ (t_r)$. Thus, if FI 40-sec schedules were programmed in each of the terminal links, the average time to reinforcement from the onset of the initial links (T) in seconds would be $30 + 40 = 70$. The model states that preference for a terminal-link schedule depends upon the *reduction* in T (overall time to reward) correlated with entry into that terminal link ($T - t$). The greater this reduction in average time to reward (relative to that correlated with entry into the other terminal link), the greater should be the preference for this schedule.

It is clear that in its present form Equation 7 would not apply directly to a situation in which reinforcement durations differed; the model lacks a parameter for magnitude of reinforcement. However, reinforcement magnitude (defined in terms of access time to the reinforcer) may be readily transformed so as to make Equation 7 applicable. The product of rate and duration of reinforcement constitutes a third variable: the number of seconds per unit time that the organism has access to the reinforcer. Assume that increases in this variable are equivalent whether they result from a decrease in the delay of reinforcement (i.e., an increase in rate of reinforcement) or an increase in the duration (or magnitude) of the reinforcer. This assumption has been made frequently, for example in Equation 1 and by Premack (1965) and Herrnstein (1970). Although it is probably in error (Fantino, Squires, Delbrück, & Peterson, 1972; Navarick & Fantino, 1975; Schneider, 1973), the assumption that rate and duration of reinforcement are equivalent in their effects upon choice is probably adequate to a rough approximation. As we shall see, that should be sufficient for our purposes. Consider the case in which the terminal links are FI 5-sec schedules terminating in 4.5 sec of reinforcement on the left and 1.5 sec on the right. The FI 5-sec schedule with 4.5 sec of reinforcement would be equivalent to an FI 1.7-sec schedule with 1.5 sec of reinforcement. The new values in Equation 7 would then be FI 1.7 sec on the left and FI 5 sec on the right.

Equation 7 may now be used to predict choice for the left terminal link. The value $T=[30+.5(1.7)+.5(5)]=33.35$ sec. The value $(T-t_l)=(33.35-1.7)=31.65$, and $(T-t_r)=(33.35-5)=28.35$. The choice proportion predicted for the left terminal link is $[31.65/(31.65+28.35)=.53]$. When these procedures are repeated with terminal-link intervals of 40 sec, the choice proportion for the left terminal link becomes .72, a substantial increase. Equation 7 thus makes an adequate ordinal prediction when the terminal-link intervals are equal and increased, in contrast to Equation 1 alone, which requires no change in preference (and is therefore incorrect in this situation).

Equation 7 (as well as Equations 1 and 3) requires reversals of preference with the schedule values employed in the first portion of the present study. For example, in one condition, the terminal link on one side consisted of an FI 2-sec schedule terminating in 1.5 sec access to grain, and the terminal link on the other side consisted of an FI 12-sec schedule terminating in 4.5 sec access to grain. Equation 7 requires a choice proportion for the larger reward of .48. When the terminal links are increased to 30 and 40 sec, respectively, Equation 7 describes a preference for the larger reward of .64, a substantial increase. Equation 7 not only requires preference reversals but also suggests that the effect of increasing terminal-link durations should be less marked than suggested by Equation 1. In particular, Equation 1 requires choice proportions of .33 and .69 for the larger hopper duration in the two extreme conditions (12- vs. 2-sec and 40- vs. 30-sec delays, respectively), a range of .36 as compared to a range of .16 for Equation 7 (.64 − .48). In fact, the obtained mean increase in choice proportions for the three subjects was .16 although there were marked individual differences.

Equation 7 thus appears to provide an adequate description of the present data at an ordinal level. One shortcoming of the model is that it cannot accurately describe exact choice proportions. This is so for at least three reasons: (a) the probable inadequacy of the assumption, made explicitly by Equation 1 and discussed above, that rate and magnitude of reinforcement are functionally equivalent; (b) the fact that the present treatment arbitrarily transforms magnitude to rate in terms of the lower of the discrepant magnitudes (somewhat different quantitative predictions would be generated by Equation 7 if the transformation were made in terms of the greater magnitude); (c) different quantitative results would be required with terminal-link schedules other than FI (Fantino & Navarick, 1974). In each case, however, accurate ordinal descriptions should be possible. Moreover, we have suggested elsewhere that a truly general model of choice must be limited to such ordinal descriptions (Fantino & Navarick, 1974; Navarick & Fantino, 1974, 1975). Based on their analysis, Navarick and Fantino (1974) suggested that:

> A major revision of behavioral theories of choice may be called for. Ordinal predictions for binary choices may prove a more realistic and fruitful goal than the prediction of exact choice probabilities. (p. 440)

The present analysis is consistent with this view and provides a framework for further theoretical development.

Previously, Equation 7 was modified and extended to simple concurrent schedules (Squires & Fantino, 1971). Equation 7 has now been extended to cases with unequal magnitudes of reinforcement in the concurrent-chains procedure. The success of Equation 7 in the present context suggests that "impulsiveness," "commitment," and "self-control" may be special cases of behavior described by this general model of choice.

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Stimulus Generalization: Some Predictions from a Model of Pavlovian Conditioning

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Three experiments examined predictions generated by incorporating a common-elements account of stimulus generalization within the Rescorla–Wagner model of conditioning. All experiments employed rats in a conditioned suppression situation. Experiments 1 and 2 found that conditioning of a similar stimulus augmented the excitation controlled by a near-asymptotic target stimulus more than did further conditioning of the target itself. Prior discrimination training between the target and the similar stimulus enlarged this effect, compared with prior discrimination between the target and another dissimilar stimulus. Nonreinforced exposure of the similar stimulus prior to its reinforcement also increased the effect. Experiment 3 examined a related prediction for inhibition. After discrimination training, extinction of the previously reinforced stimulus revealed more inhibition to the previously nonreinforced stimulus when those two stimuli were more similar. These outcomes are consistent with deductions from the present model and encourage further testing of its expansion to the case of stimulus generalization.

Over the last several years, we have described a reasonably successful model of Pavlovian conditioning (Rescorla & Wagner, 1972; Wagner & Rescorla, 1972). This model accounts for a broad range of conditioning phenomena by assuming that conditioned stimuli (CSs) which precede an unconditioned stimulus (US) are restricted in the total amount of conditioning which they may gain from that US. Many applications of this model have described how that limited amount of conditioning is shared by separable stimuli which are members of a compound preceding the US. The intention of the present paper is to explore some empirical consequences of extending that description to a case without such separable stimuli.

The formal statement of this model describes how the associative strength (V) of individual stimuli changes as a result of a single trial. Thus if an AB compound is followed by an effective US, the changes in the strengths of the A and B elements are $\Delta V_A = \alpha_A\beta(\lambda - V_{AB})$ and $\Delta V_B = \alpha_B\beta(\lambda - V_{AB})$, where λ is the asymptote of conditioning the US will support and α and β are rate parameters dependent, respectively, upon the CS and US. The novel feature is this model's assertion that conditioning of the individual stimuli depends upon the total strength of the compound (V_{AB}), which is assumed to equal $V_A + V_B$. It is this feature which forces separable stimuli to share the limited (λ) conditioning of the US and enables the model to make its most interesting predictions.

Although this description was designed to account for the conditioning of separable events, it has neglected the problem of stimulus generalization among those events. One way to rectify that oversight is to employ the historically popular notion of common elements shared by similar stimuli. Thus one can conceptualize any two stimulus events, such as a high- and low-frequency tone, as compounds, AX and BX, where X represents their shared features and A and B their unique features. Making such an assumption within the present model has many consequences in common with

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those of other models (see Rescorla, Note 1). For instance, excitatory conditioning of a high-frequency tone should generalize to a low tone to the degree that the shared X features have gained some associative strength. As seems intuitively reasonable, the degree of overlap between high and low tones, and thus the degree of generalization, depends upon the salience of the shared features (α_X).

But the fixed-capacity feature of this model also enables it to make some novel predictions about stimulus generalization. The present paper provides preliminary evidence on one set of particularly paradoxical predictions. According to this model, there should be circumstances under which the strength of a target stimulus receives greater modification by treatment of a similar stimulus than by treatment of the target itself. The model, when combined with a common-elements assumption, details procedures in which, contrary to normal expectation, treating a stimulus directly is not the best way to modify its associative strength. Experiments 1 and 2 explore two such instances for excitatory conditioning, whereas Experiment 3 examines a parallel case for conditioned inhibition. All three experiments employed rat subjects in a conditioned fear situation in which suppression of ongoing appetitive behavior is the measure.

EXPERIMENT 1

Consider a situation in which a high-frequency tone (AX) has been conditioned to near-asymptotic value, that is, in which V_{AX} may be assumed to be close to λ. Presentation of a similar low-frequency tone (BX) would be expected to give some, but less, conditioned response, because of the shared X features. That incomplete response indicates that BX is itself subasymptotic and thus capable of receiving increases in associative strength if it were reinforced. Within the Rescorla–Wagner model, such increasing of V_{BX} would result in individual increments in V_B and V_X. Consequently, such $BX+$ training would make V_X stronger than it was after simple $AX+$ training. If we then tested AX, we would anticipate an augmented response to it, since $V_{AX} = V_A + V_X$. Furthermore, that response should be greater than that which would have been observed if we had simply continued $AX+$ training, since V_{AX} has already been trained until it is near asymptotic at λ. Thus, the model anticipates that, despite its near-asymptotic training, a high tone could nevertheless receive increments in strength through the training of a low tone which generalizes to it. That high tone should gain more through generalization than through additional direct training.

Furthermore, the model anticipates manipulations which could augment that difference. Any treatment which reduces the share of conditioning which X receives on the initial $AX+$ trials should leave more room for V_X to increase on subsequent $BX+$ trials, and thus permit more generalization back to AX. One way to accomplish this would be to replace initial $AX+$ training by a discrimination between $AX+$ and $BX-$. That discrimination should produce a well-trained AX, but relatively more of the strength should reside in A because of the nonreinforcement of X in compound with B. If such a discrimination were then followed by $BX+$ training, there would be considerable augmentation of AX. Consequently, the model anticipates that prior discrimination training between high and low tones should augment the generalization of excitation from the subsequently reinforced low tone to the asymptotic high tone.

This experiment investigates both of these possibilities. Two groups of rats received discriminated fear conditioning in which a high tone was reinforced and a low tone nonreinforced. Then one group received additional reinforcements of the high tone while the other received conditioning of the low tone. Finally, both groups received extinction generalization tests with the high tone. Another set of groups received similar treatment except that the initial discrimination was between a light and the high tone, two stimuli which presumably share few features. This procedure should expose the animals to discrimination training, but without especially reducing the amount of high-tone conditioning which accrues to X, the elements shared by the high and low tones. After this treatment, low-tone conditioning should augment the excitation controlled by the high tone but to a lesser degree than in the preceding two groups.

Method

Subjects and apparatus. The subjects were 28 male Sprague-Dawley rats about 90 days old at the start of the experiment. They were maintained throughout at 80% of their normal body weight.

The experimental chambers consisted of seven identical Skinner boxes 22.9 × 20.3 × 20.3 cm. Each chamber had a recessed food magazine in the center of the end wall and a bar to the left of the magazine. The floor of the chamber was composed of .48-cm stainless steel rods spaced 1.9 cm apart. This grid could be electrified through a relay-sequence scrambler from a high-voltage high-resistance shock source. The two end walls of the chamber were aluminum; the side walls and top were clear Plexiglas. Each Skinner box was enclosed in a sound- and light-resistant shell. Mounted on the wall of this shell were a 6½-W bulb and two speakers. The speakers permitted the presentation of a 1,800- and a 250-Hz tone, each of which measured about 75 db. re 20 $\mu N/m^2$ against a background level of 62 db. Experimental events were controlled and recorded automatically by relay equipment located in an adjoining room.

Procedure. In the first session, all rats were magazine trained automatically with food pellets delivered on a variable time 1-min schedule. In addition, each bar press yielded a food pellet. This session continued until the animal had emitted 50 bar presses; shaping was used if necessary. Starting with the second experimental day, all sessions were 2 hr long and all involved food reinforcement earned on a variable interval (VI) schedule. For the first 20 min of Day 2 the schedule was VI 1 min; thereafter, it was VI 2 min. Simple VI training continued for five sessions.

On the next 2 days the stimuli later to be used in conditioning were pretested. On each day, four nonreinforced CSs of 2-min duration were superimposed on the bar pressing. On the first pretest day, two of the stimuli were 1,800-Hz tones (A) and two were a flashing of the houselight at the rate of 2/sec (C). On the second day, the stimuli were two 250-Hz tones interrupted at the rate of 1/sec (B).

During the next 7 days, initial discriminative conditioning was administered. Two groups (A+,B−/A+ and A+,B−/B+) received two reinforced presentations of the high tone (A) and four nonreinforced presentations of the low tone (B) each day. Two groups (A+,C−/A+ and A+,C−/B+) received two high-tone reinforced and four light (C) nonreinforced trials per day. All stimuli were 2-min and terminated, where appropriate, in a .5 mA .5 sec footshock.

On the next 2 days, simple fear conditioning was carried out. Each day contained four reinforced trials. For Groups A+,B−/A+ and A+,C−/A+ the CS was the high tone (A+); for Groups A+,B−/B+ and A+,C−/B+ it was the low tone (B+).

Finally, extinction test sessions with the high tone were administered on the next 4 days. Each day, four nonreinforced presentations of the 1,800-Hz tone were superimposed on bar pressing. It is the differential suppression to the high tone during this test which constitutes the principal datum.

Throughout, the measure of conditioned fear was the amount of suppression of bar pressing produced by a stimulus presentation. In order to attenuate the effects of individual differences in overall rate of responding, the results are plotted in terms of a suppression ratio. This ratio has the form $x/(x+y)$, where x is the rate of responding during the CS and y is the rate in a comparable period prior to CS onset. Thus, a ratio of zero indicates no responding during the CS (good conditioning), whereas one of .5 indicates similar rates of responding during the CS and pre-CS periods (little conditioning).

Results and Discussion

The course of the initial discrimination between the high tone and either the light (Groups A+,C+/A+ and A+,C+/B+) or the low tone (Groups A+,B+/A+ and A+,B+/B+) is shown on the left-hand panel of Figure 1. The high tone rapidly acquired substantial suppression in both treatments and by the end of training neither nonreinforced stimulus evoked substantial suppression. However, there was initial generalization to the low tone in Groups A+,B−/B+ and A+,B−/A+, but little generalization to the light in Groups A+, C−/B+ and A+,C−/A+. This greater generalization to the low tone is revealed by its reliably greater suppression on Days 3 and 4 of discrimination training (Mann-Whitney $U = 26$, $p < .002$).[1]

The development of fear during the next 2 days of simple conditioning is shown in the right-hand panel of Figure 1. Those groups which received additional training to the high tone (A+,B−/A+ and A+,C−/A+) continued to show substantial suppression to it. Group A+,B−/B+, which had a history of nonreinforced low-tone presentation, showed no initial fear of that stimulus but rapidly acquired suppression over the eight trials of B+ conditioning. Group A+,C−/B+ showed substantial initial generalization to the low tone and then developed additional suppression. This generalization indicates that without discrimination between low and high tones, conditioning of the high tone resulted in substantial fear conditioning to the elements shared by the two tones. The reliably greater initial suppression to the low tone in Group A+, C−/B+ compared with Group A+,B−/B+ (for Trials 1 and 2, $U = 0$, $p < .01$) indicates that the treatments were successful in generating differential amounts of conditioning to those shared elements.

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[1] One-tailed U tests are employed through this report.

The data of primary interest, from the final extinction test with the high tone, are shown in Figure 2. On the initial test day, both groups which had received conditioning with the low tone exhibited more suppression than did their comparison groups which had received additional conditioning of the high tone. A Wilson nonparametric analysis of variance confirmed the greater suppression in the two B+ groups ($\chi^2 = 5.1$, $p < .05$). Individual Mann-Whitney *U* tests revealed Group A+,C+/B+ to be reliably different from Group A+,C+/A+ ($U = 10.5$, $p < .05$) and Group A+,B+/B+ to be more suppressed than Group A+,B+/A+ ($U = 8$, $p < .05$). Neither the nature of the prior discrimination training nor the interaction between that training and the nature of the stimulus individually reinforced produced statistically reliable effects on Test Day 1. Similar data and statistics were observed for the first two test trials of that day. Despite the fact that suppression was generally too substantial to detect reliable differences on Trial 1 alone, this suggests that the Test Day 1 results were not solely a product of extinction testing.

Over the course of testing, the difference between the A+,C− groups disappeared while that between the groups with a prior A+,B− discrimination persisted. By Test Day 2, it made little difference in those groups with a history of high–light discrimination whether the high or low tone had received additional conditioning. But for those groups with prior high–low discrimination training, suppression to the high tone was greater with intervening low-tone conditioning. These observations are confirmed by the interaction between discrimination history and stimulus conditioned ($\chi^2 = 5.1$, $p < .05$) as well as continued overall superiority in groups which had received B+ training ($\chi^2 = 4.2$, $p < .05$). Individual Mann-Whitney *U* tests indicated that Group A+,B−/B+ continued to show more suppression than both Group A+,B−/A+ and Group A+,C−/B+ (*U*s < 9, *p*s < .05).

These results are in general agreement with those predicted on the basis of combining a common-elements approach with the Rescorla–Wagner model. Training on a stimulus capable of generalizing to a target stimulus produced greater subsequent suppression to that target than did further training of the target itself. Furthermore, that greater effect of a generalizing stimulus was enhanced when it, rather than a dissimilar stimulus, had a prior history of

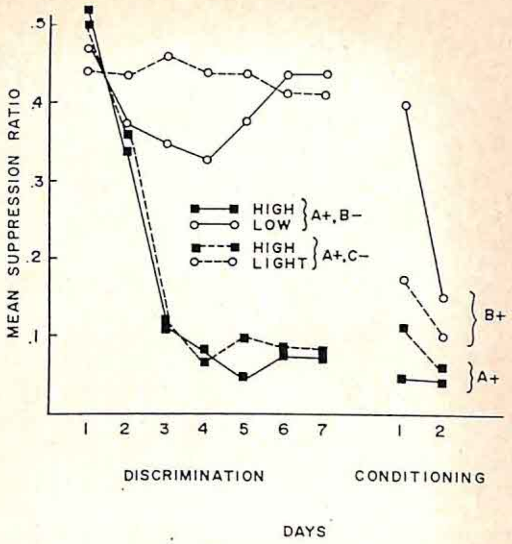

FIGURE 1. Discrimination and subsequent conditioning phases of Experiment 1. (The left-hand panel shows the development of discrimination between the reinforced high tone and either the low tone [Groups A+,B−/A+ and A+,B−/B+] or the light [Groups A+,C−/A+ and A+,C−/B+]. The right-hand panel shows subsequent reinforcement of the high tone, A+, in Groups A+,B−/A+ [squares connected by solid lines] and A+,C−/A+ [squares connected by dashed lines], and of the low tone, B+, in Groups A+/B−/A+ [circles connected by solid lines] and A+,C−/B+ [circles connected by dashed lines]. A, B, and C indicate high tone, low tone, and light, respectively.)

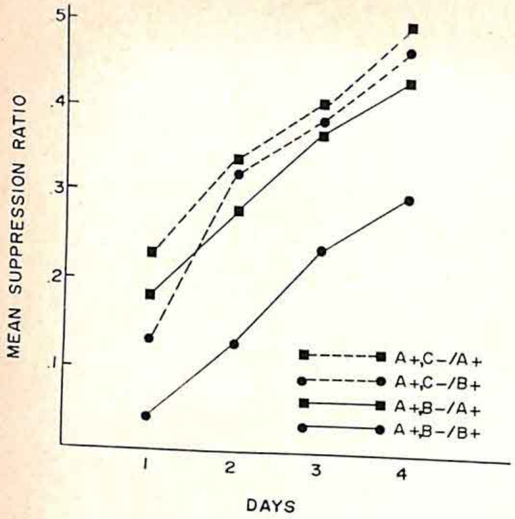

FIGURE 2. Extinction generalization test of Experiment 1. (All animals received four nonreinforced high tones (A) per day. Groups are identified by their two prior treatments. B and C indicate low tone and light, respectively.)

being discriminated from that target. Neither of these outcomes seems to follow from traditional theories which have incorporated the common-elements assumption (e.g., Atkinson & Estes, 1963).

EXPERIMENT 2

According to the present account, the augmentation in the strength of an asymptotic target stimulus produced by reinforcing a similar stimulus depends upon the incomplete conditioning of the elements shared by the two stimuli. Thus, any treatment which reduces the proportion of the initial AX conditioning which X gains should also enhance the ability of subsequent BX reinforcement to increase the strength of AX. In Experiment 1 this was accomplished by explicit discrimination training in which X was an irrelevant cue. Experiment 2 explores an alternative manipulation, the reduction of the salience of X, and in addition provides a replication of the basic effect reported in Experiment 1.

With explicit stimulus compounds, reducing the salience of one separable element reduces its share of the compound's strength (Kamin, 1969; Miles & Jenkins, 1973). Within the Rescorla–Wagner model, for instance, the asymptotic proportion of conditioning to X as a result of $AX+$ training is $\alpha_X/(\alpha_A + \alpha_X)$. Clearly in that theory, the lowering of α_X reduces the magnitude of V_X. A treatment which apparently reduces the salience of a stimulus is simple repeated nonreinforced presentation prior to conditioning (e.g., Lubow, 1973; Reiss & Wagner, 1972; Rescorla, 1971). Such presentation results in so-called latent inhibition which is indexed by subsequently retarded conditioning. Despite the fact that in the present context X is not a separable stimulus, it should be possible to reduce its salience relative to A prior to AX reinforcement. Since X represents exactly those stimulus aspects of the AX target which are shared with the similar stimulus, then repeated presentation of the latter (BX) should reduce the salience of X as well as of B. If AX is then reinforced, A should gain a larger proportion of the conditioning, leaving room for $BX+$ trials to augment X to a larger extent. Consequently, preexposure to a low tone should enhance the ability of subsequent conditioning of that tone to strengthen a near-asymptotic high tone.

Method

Subjects and apparatus. The subjects were 32 male Sprague-Dawley rats maintained at 80% of their normal body weight. The apparatus consisted of eight Skinner boxes identical in all essentials to those of Experiment 1.

Procedure. After initial bar-press training identical to that of Experiment 1, all sessions were 2 hr long and all involved bar pressing on a VI 2-min schedule. Simple VI training continued for 5 days. Thereafter the animals were divided into four groups of eight each, and the groups were identified according to their treatments in three successive phases.

On the next day, the first phase, preexposure, began. On each of 3 days, the animals in Groups B−/A+/B+ and B−/A+/A+ each received eight 2-min presentations of the interrupted 250-Hz (B−) tone. The animals in Groups −/A+/B+ and −/A+/A+ received no programmed stimulus events during these three preexposure sessions.

During the 3 days of the second phase, all animals received fear conditioning of the 1,800-Hz high tone (A+). On each day, four 2-min stimuli each terminated in a .5-mA .5-sec footshock.

The next 3 days constituted the third phase. On each day, all animals received four 2-min stimuli, each ending in footshock. For the animals in Groups B−/A+/A+ and −/A+/A+ the stimulus was a high tone (A), whereas for those in Groups B+/A+/B+ and −/A+/B+, it was the low tone (B).

On the next day, all animals received simple bar-press training, to ensure that their response rates fully recovered from any general suppressive effects of shock. Finally, on each of the next 3 days extinction tests were given with the high tone. All animals received four nonreinforced 2-min presentations of the high tone on each test day.

Results and Discussion

Following preexposure to the low tone, initial conditioning of the high tone (A) was slightly retarded. On the first two conditioning trials, the mean suppression for the preexposed groups was .43, whereas that for the others was .38, a difference which was marginally reliable ($U = 83$, $p < .05$). One plausible interpretation is that "latent inhibition" generalized from the low to the high tone, reducing its salience. By the end of high tone conditioning, all four groups were substantially and equivalently suppressed, but with Phase 3 low-tone conditioning a related difference appeared. During this stage, Group B−/A+/B+ showed less suppression on Trials 1 and 2 than did Group A−/A+/B+. The mean suppression ratios were .29 and .09 respectively ($U = 15$, $p < .05$). That difference presumably reflects the protection which preexposure has given the low tone against generalization of fear from the high tone. These results thus suggest that preexposure of the low tone reduced the proportion of the high-tone conditioning gained by the common elements.

The data of most interest, from the extinction test presentations of the high tone, are shown in Figure 3. Within both the preexposed and the nonpreexposed pairs of groups, conditioning of the low tone (B) produced more suppression to the high tone (A) than did further training of the high tone itself. However, the difference between A and B conditioning persisted throughout testing in the preexposed groups but was attenuated in the nonpreexposed groups. Thus, a Wilson nonparametric analysis of variance for Test Day 1 revealed only a reliable effect of which tone received Phase II conditioning ($\chi^2 = 4.5$, $p < .05$); neither the apparent effect of preexposure nor the interaction was reliable, ($\chi^2 = .5$, 0, respectively). By the final test day, however, a Wilson test revealed that only the interaction between preexposure and stimulus conditioned was reliable ($\chi^2 = 4.5$ $p < .05$). On this test day, Group B−/A+/B+ continued to be more suppressed than Group B−/A+/A+, whereas the comparable difference between the nonpreexposure groups was no longer evident.

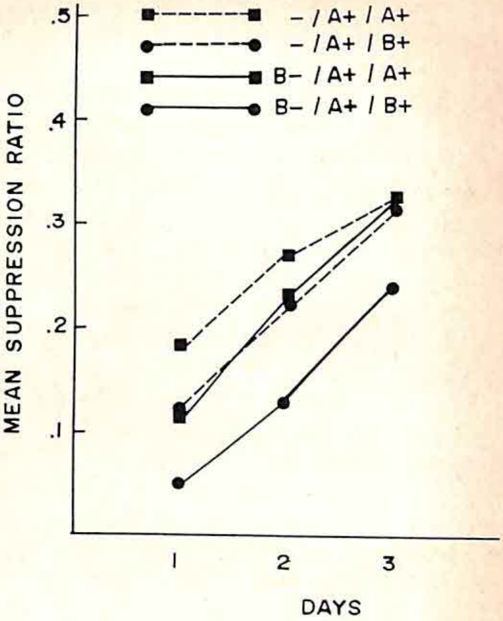

FIGURE 3. Extinction generalization test of Experiment 2. (All animals received four nonreinforced high tones per day. Groups are identified by their three prior treatments. A and B indicate high and low tones, respectively.)

Thus these results substantiate those of Experiment 1. After asymptotic conditioning on the high tone, reinforcement of the low tone resulted in more high-tone suppression than did further training on the high tone itself. Furthermore, that effect was enlarged when the low tone had received less generalized strength from the high tone because of prior latent inhibition treatment.

EXPERIMENT 3

The preceding two experiments point to interesting implications of the present analysis for conditioned excitation. But the analysis also has importance for inhibition. According to this approach, successful completion of an $AX+,BX-$ discrimination occurs because B becomes sufficiently inhibitory to reduce responding to BX, despite the excitation acquired by the shared X on $AX+$ trials. Furthermore, the degree of inhibition required of B to complete the discrimination will be larger the more salient is X and the greater its consequent excitation (see Rescorla & Wagner 1972).

For those cases in which X represents shared stimulus features, this means that the more difficult the discrimination due to stimulus similarity (and the consequently greater α_X) the more inhibition should accrue to those elements unique to the nonreinforced stimulus. Ordinarily this inhibition would be difficult to observe because of the concurrent presence of the excitatory X; indeed, with substantial overlap, the development of the discrimination would appear to proceed quite slowly. But if the shared elements should lose their excitation after discrimination training, then we would expect the B elements to reveal their inhibition in relatively pure form. One way to accomplish this would be to extinguish the previously reinforced AX after accomplishing the $AX+$,BX$-$ discrimination. If that treatment removed the excitation controlled by X, then we would expect to see the greater inhibition controlled by B in those cases where X is more salient.

The present experiment provides an initial examination of this possibility. A discrimination was taught in which a high tone was nonreinforced. For one group a similar low tone was reinforced whereas for another group a relatively dissimilar light was reinforced. Then in each case the previously reinforced stimulus was extinguished and finally an attempt was made to condition the high tone. In the present account the unique features of the high tone should control greater inhibition in the case where it was discriminated from the more similar low tone; that, in turn, should slow the acquisition of excitatory conditioning.

Method

Subjects and apparatus. The subjects were 16 male Sprague-Dawley rats about 90 days old at the beginning of the experiment and maintained at 80% of their normal body weight. The apparatus consisted of four Skinner boxes identical in all essentials to those of previous experiments.

Procedure. After initial bar-press training identical to that of Experiment 1, all sessions were 2 hr long and all involved food reinforcement earned on a VI 2-min schedule. After 5 days of simple VI training, a 2-day pretesting sequence was given. On the first pretest day, each animal received four 2-min nonreinforced presentations, two of the 1,800-Hz high tone and two of the interrupted 250-Hz low tone. The second pretest day was identical except that the flashing of the houselight replaced the low tone.

During the next seven sessions, discriminative fear conditioning was administered. On each day, two reinforced and four nonreinforced stimuli were given. For all animals the nonreinforced stimulus consisted of a 2-min high tone; for the animals in Group A−,B+/B− the reinforced stimulus was a 2-min presentation of the 250-Hz tone (B), whereas for Group A−,C+/C− it was a 2-min presentation of the flashing houselight (C). For all animals the unconditioned stimulus was a .5-mA .5-sec footshock, present at CS termination.

For the next six sessions, all animals received extinction with the previously reinforced stimulus. On each day, six nonreinforced lights were administered to Group A−,C+/C− and six low tones to Group A−,B+/B−.

Finally, on next 9 days the animals received a retardation assessment test of the inhibition possibly controlled by the high tone. For all animals four 2-min high tones were presented during each session, two reinforced and two nonreinforced. This partial reinforcement schedule was employed to generate slower acquisition which might more easily reveal between group differences. The data of interest are the rates of acquisition of fear to the high tone during this test sequence where differences were anticipated due to differences in degree of inhibition (Rescorla, 1969).

Results and Discussion

Discrimination proceeded similarly to that of previous experiments, with the low–high differentiation being slower than the light–high one. By the end of discrimination training, however, both groups exhibited virtually complete suppression to their reinforced stimulus but little or no suppression to the high tone. Similarly, little difference appeared during extinction of the previously reinforced stimulus.

The data of most interest are the rates of fear acquisition during the retardation test, shown in Figure 4. Although neither group showed initial suppression to the high tone, both developed fear over the course of con-

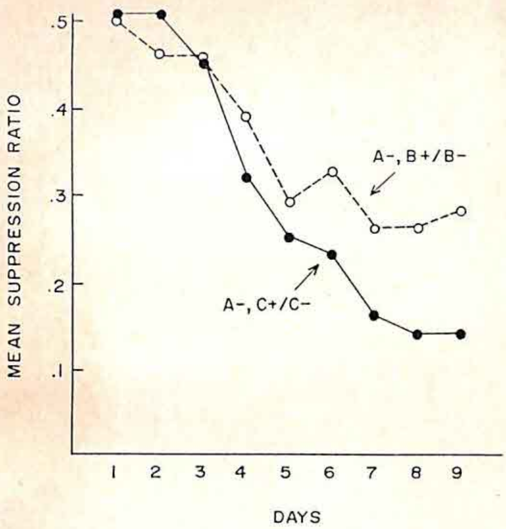

FIGURE 4. Retardation test of Experiment 3. (All animals received excitatory conditioning with the high tone (A). Groups are labeled by their previously reinforced stimulus. B and C indicate low tone and light, respectively.)

ditioning with shock. More interestingly, the rate of acquisition was retarded in Group A−,B+/B− compared with Group A−, C+/C−. Over the last three conditioning sessions that difference was reliable ($U = 10$, $p < .01$).

This finding suggests that the high tone acquired fear more slowly when it had been discriminated from a similar stimulus than when it had been discriminated from a dissimilar stimulus. In terms of the present account, that difference results from the greater inhibition controlled by the high tone in the former case. But it should be noted, of course, that the present findings are not conclusive in demonstrating the inhibitory character of the high tone. For such a conclusion, one would require at least the additional results of a summation test procedure showing the high tone to reduce the response otherwise controlled by an excitatory stimulus (Rescorla, 1969). Nevertheless, these results provide initial support for the present analysis.

GENERAL DISCUSSION

The results of these three experiments encourage the adoption of a common-elements assumption within the Rescorla–Wagner model. That assumption is clearly capable of describing in a qualitative way many of the facts of stimulus generalization. But it additionally makes a number of interesting counterintuitive predictions, like those tested here. It accurately points to some instances in which more conditioning can be produced to a target stimulus by reinforcing some other stimulus than by reinforcing the target itself. Furthermore, it suggests circumstances under which prior discrimination training between two stimuli enhances the generalization between those stimuli of subsequent conditioning.

It is of interest to compare the present formulation and data to those recently reported by Blough (1975). Using a steady-state discrimination training situation with instrumental key pecking in pigeons, Blough reported results related to those described in Experiments 1 and 2 here. In that procedure, many stimuli along a continuum received a low rate of reinforcement. Superimposed on that background, one stimulus received additional nonreinforced trials (S−). Although Blough found a minimum in the generalization function at S−, he also found "shoulders" in the function. These took the form of greater responding to stimuli at a moderate distance from S− than to equally well reinforced stimuli more dissimilar to it. As Blough points out, with such a procedure stimuli very close to S− should become excitatory, but in such a way that their elements which are not shared with S− are especially strong. Those elements, however, are likely to be shared by stimuli at a moderate distance from S− and thus to contribute substantially to responding during those stimuli and so produce shoulders.

In accounting for these data, Blough also described a particular elaboration of the Rescorla–Wagner model using a stimulus elements assumption. By also making a number of detailed additional assumptions he was able to generate a model of considerable power. The present approach, on the other hand, leaves unspecified many aspects of the functioning of the hypothesized stimulus elements. Instead it only points to the general character of a common-elements assumption within the context of the present model. For this reason it lacks much of the power of Blough's formulation. But at the same time it emphasizes the point that a broad class of models would generate certain interesting qualitative predictions, regardless of their individual additional assumptions.

The present results also make an interesting methodological point about the hypothesized common elements. Our inability to separately present the shared and unique elements of a set of stimuli does not prevent them from being manipulated to make differential predictions. As illustrated in Experiment 2, for instance, certain selections of treatments can permit one to differentially affect those shared elements despite their inseparability from one or another unshared element. In this respect these stimulus elements are similar to the "unique" stimulus which we have elsewhere suggested as important in the account of configural conditioning. Those stimuli too are conceptual in nature and do not admit of separate presentation. Yet they too, taken in the context of a general theory, can permit testable deductions. Indeed, we have previously investigated a set of propositions analogous to those described here for those unique elements (Rescorla, 1973).

Finally, the results of Experiment 3 may go some distance toward resolving an apparent paradox involved in the role attributed to inhibition in discrimination learning. A number of authors (e.g., Konorski, 1948; Pavlov, 1927) have claimed that the more similar are CS+ and CS− the more inhibitory does the latter become. Yet that claim seems consonant neither with the slower discrimination normally observed with more similar stimuli nor with the frequent observation of continued behavior to those CS−s which are similar to CS+. The present analysis suggests that, although a CS− which is similar to CS+ may persist in generating some behavior, nevertheless, its nonshared elements may control more inhibition than do those of dissimilar CS−s. With procedures like those employed here, in which the shared elements are reduced in power, that greater inhibition may be revealed.

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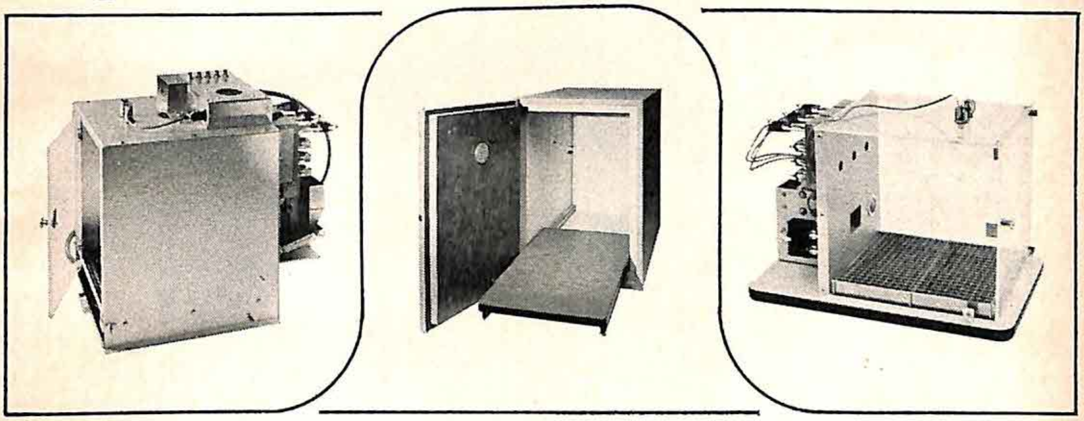

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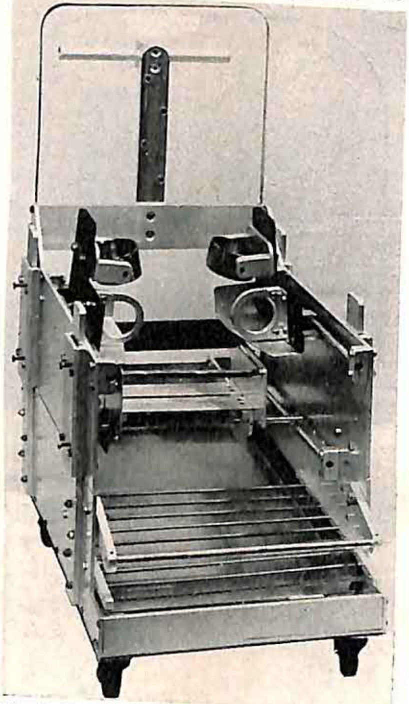

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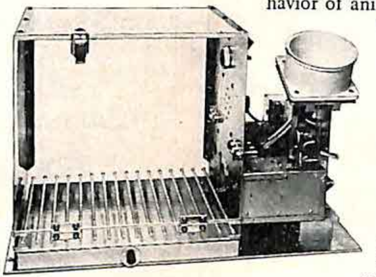

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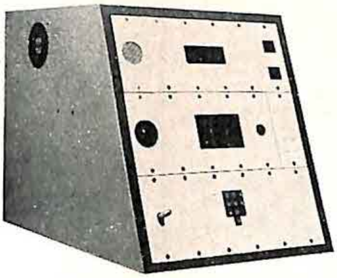

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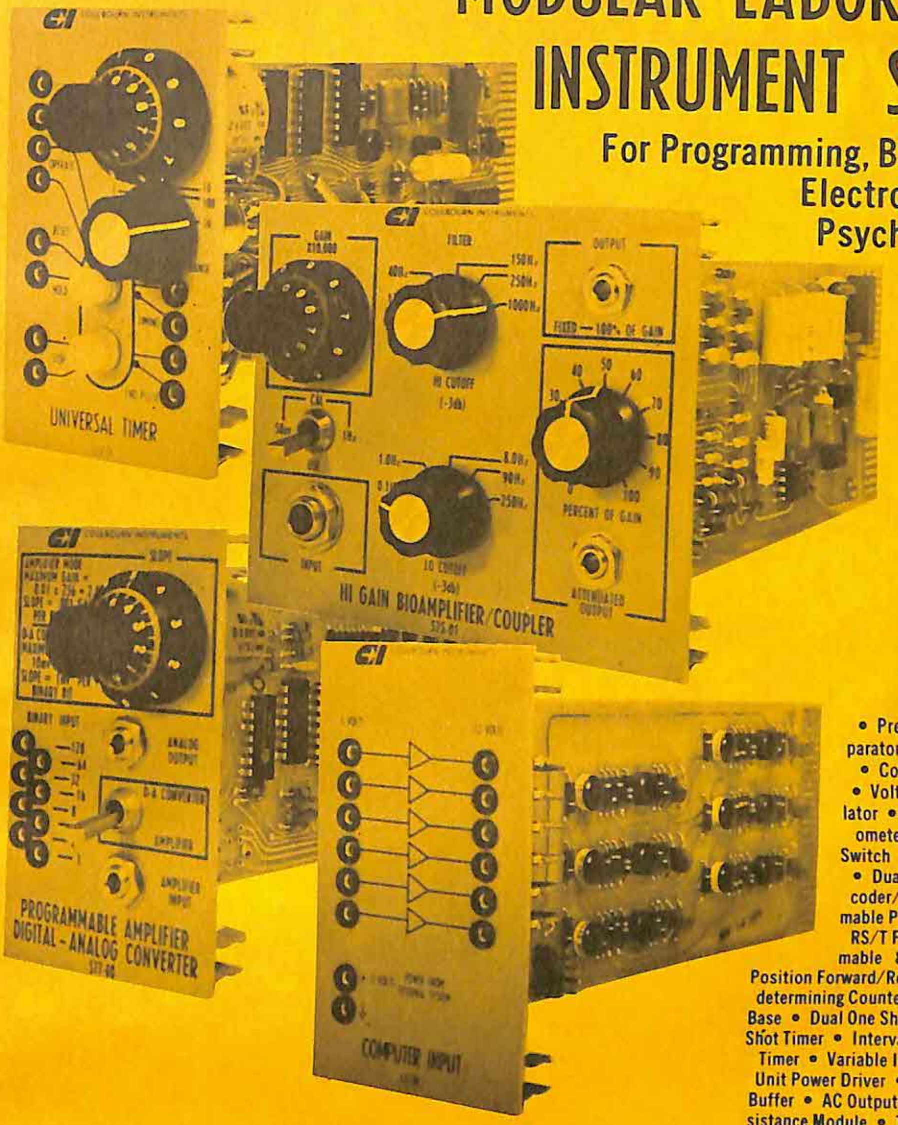

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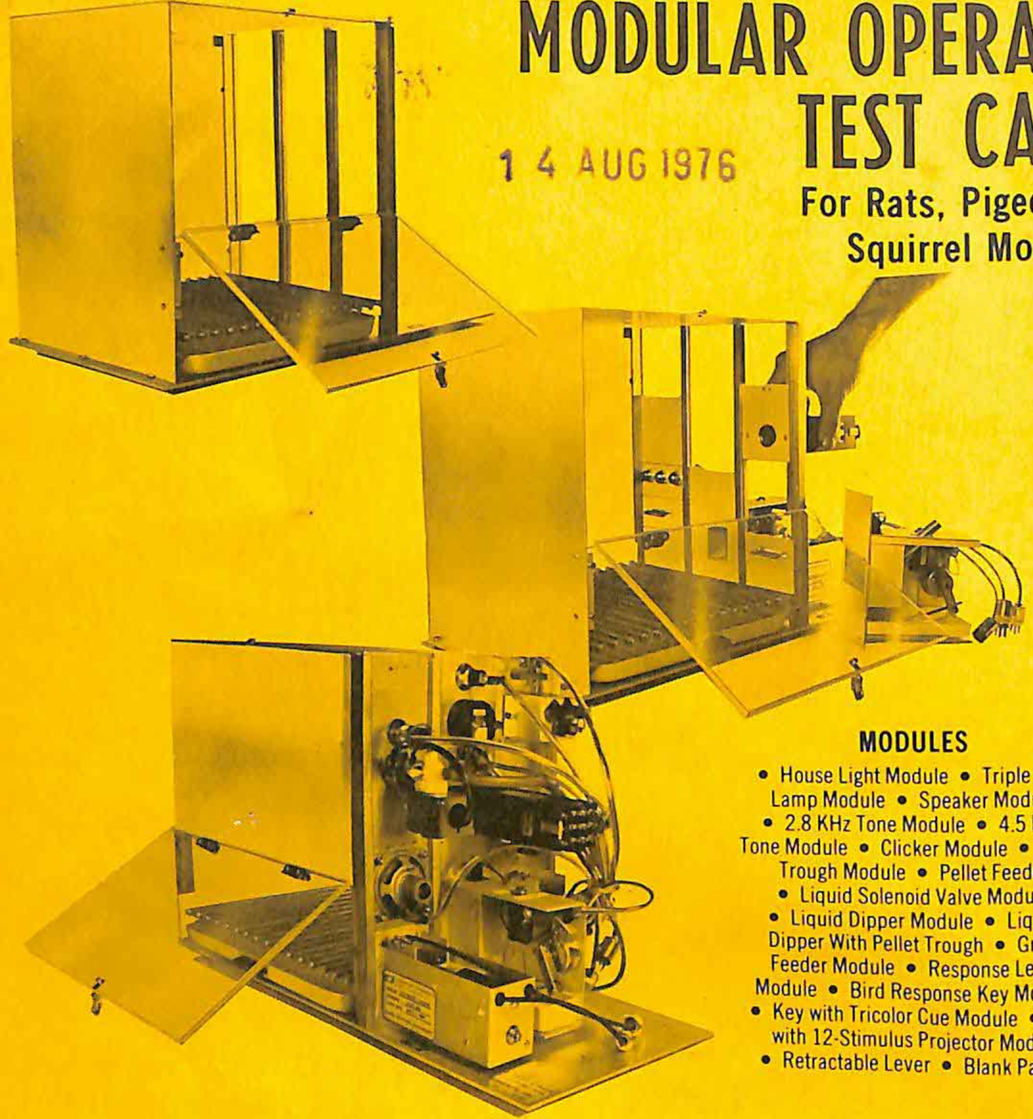

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American Psychological Association **Vol. 2, No. 2, April 1976**

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Remembrance of Places Passed: Spatial Memory in Rats

David S. Olton and Robert J. Samuelson
The Johns Hopkins University

Rats were tested on an eight-arm maze in a paradigm of sampling with replacement from a known set of items until the entire set was sampled. The first three experiments demonstrated that the animals performed efficiently, choosing an average of more than seven different arms within the first eight choices, and did not utilize intramaze cues or consistent chains of responses in solving the task. The second three experiments examined some characteristics of the rats' memory storage. There was a small but reliable recency effect with the likelihood of a repetition error increasing with the number of choices since the initial instance. This performance decrement was due to interference from choices rather than just to the passage of time. No evidence was found for a primary effect. The data also suggest that there was no tendency to generalize among spatially adjacent arms. The results are discussed in terms of the memory processes involved in this task and human serial learning.

When distinctive exteroceptive discriminative stimuli are consistently associated with a particular spatial location, rats preferentially use these stimuli for discrimination learning, a phenomenon that is usually referred to as "place learning." If place learning can be used to solve a discrimination problem, rats learn very rapidly. If place learning cannot be used to solve a discrimination problem, rats learn slowly and almost invariably adopt a "position habit" or "spatial hypothesis" before finding the correct solution. (Relevant literature reviews may be found in Gleitman, 1955; Kimble, 1961, p. 223; Olton & Samuelson, 1974; Sutherland & Mackintosh, 1971; Tolman, Ritchie, & Kalish, 1946; Woodworth, 1938, p. 124). Also, spontaneous alternation in a T maze is strongly influenced by spatial cues (Douglas, 1966), and during avoidance behavior rats preferentially associate shock with spatial location rather than with visual or auditory cues (Olton, 1973).

In spite of the ubiquitous nature of place learning, most experiments have treated place learning as a factor to be controlled and have chosen to assess rats' cognitive abilities by making place learning impossible. In contrast, the present experiment was designed to permit place learning and to utilize this learning to assess the capacity of rats to discriminate, remember, and process information derived from place learning in searching for food. To accomplish this goal, we developed a new experimental testing paradigm, one which requires sampling with replacement from a known set of items until the entire set is sampled. In order to establish this procedure, an appa-

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This research was supported in part by National Institute of Mental Health Grant 1-R01-MH-23213 to David S. Olton. The authors express their appreciation to E. Blass, J. Deese, H. Egeth, S. Hulse, B. Green, Jr., and R. M. Olton, Jr., for their assistance in the preparation of this manuscript.

Requests for reprints should be sent to David S. Olton, Department of Psychology, The Johns Hopkins University, Baltimore, Maryland 21218.

ratus was constructed which required the rat to return to the same central choice point after each choice so that the entire set of choice alternatives (including the ones already chosen) was presented on each trial. The reinforcement contingencies were arranged so that the first choice of each alternative was reinforced, while subsequent choices were not, making the optimal behavior choosing each alternative once until all alternatives were chosen. This procedure is in distinct contrast to the typical maze learning task in which choice alternatives are presented sequentially and the animal is faced with only one subgroup (usually a pair) of the entire set of alternatives at any given time.

The first three experiments were designed to investigate the general characteristics of performance in this paradigm and to assess the relative importance of extramaze cues, intramaze cues, and response chains in order to determine the type of strategy used by the rats. The subsequent experiments were then conducted to describe the characteristics of the memory processes that underlie performance.

EXPERIMENT 1

Method

Subjects. The subjects were six experimentally naive male albino rats that weighed 300–325 g at the start of testing.

Apparatus. The apparatus was a radial eight-arm maze as diagrammed in Figure 1. The center platform was 34 cm wide. All arms were the same length (86 cm) but various widths (1.3, 2.5, 3.8, 5.0, 6.3, 7.5, 9.3, and 10.0 cm). The entire apparatus was made of wood and was approximately 50 cm above the floor. The testing room was well illuminated by ceiling lights unless otherwise noted. There were numerous visual cues scattered around the room, including a sink, table, columns, and so on.

Procedure

Exploration. During the first 9 days all rats had ad-lib food and water in the home cage. On each day they were given approximately 15 min. to explore the apparatus; no food was present in the apparatus during this time.

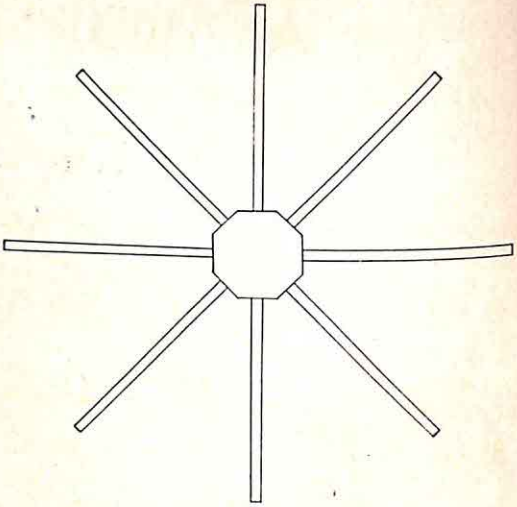

FIGURE 1. Diagram of a top view of the apparatus.

Free choice. For the rest of the experiment, all rats were placed on a food deprivation schedule to bring body weights down to 85% of ad lib; daily feeding took place within ½ hr. after testing. During the next 10 days (Days 10–19), reinforcement was provided by pieces of Purina Lab Chow that weighed approximately .1 g each. At the start of each test session, one piece of food was placed at the end of each arm; reinforcement was not replaced during the test so that a maximum of eight pieces of food could be obtained. Each rat was placed on the center platform of the apparatus and allowed to make 16 choices or given a total of 10 min. if 16 choices were not made. A choice was defined as the rat proceeding to the end of an arm.

Rebait. During the next 5 days (Days 20–24), reinforcement was replaced on one of the eight arms after the rat had chosen that arm and returned to the center platform. The actual arm rebaited varied, but in all cases it was an arm chosen within the first six choices. If the presence of food at the end of an arm influenced the rat's choice of that arm, the rat ought to return to the rebaited arm after fewer choices than in the free choice procedure.

Added odor. During the next 3 days (Days 25–27), the apparatus was liberally sprinkled until wet with either Old Spice or Mennen aftershave lotion. The lotion was applied to both the center platform and to all of the arms just prior to testing each animal. If the odor of food at the end of an arm or some kind of "odor trail" within the apparatus influenced the rat's choice behavior, then choice accuracy here should be lower than that in the free choice procedure.

Results

Exploration. During the first day of testing, all rats moved freely about the center platform of the apparatus but rarely went onto the arms. Over the next few days they began to venture out onto the arms until they would readily run from the center platform to the end of an arm and back again. Typically, the rats would pause at the edge of the platform before entering an arm and orient toward the end of the arm while rapidly moving their vibrissae. This orienting behavior was followed either by withdrawal to the center of the apparatus and repetition of the orienting behavior at another arm or by a choice of the arm. On other occasions, the rats would run rapidly across the center platform and without pause run down an arm; this type of behavior was usually observed only on the first few choices. In almost all cases, once a rat had placed all four legs onto an arm, it proceeded to the end of the arm before turning around. There was a marked preference for the wider arms, and rats rarely chose the three most narrow arms.

Free choice. The general activity level of the rats increased markedly from the preceding exploration conditions. At first, some food pellets were left on an arm even though the rat had placed its nose very close to the pellet and presumably noticed its presence. Within a few days, all food pellets were taken from the arms and consumed, usually in the center platform. As the rats became more familiar with the procedure, they would often eat either at the end of an arm or while running along the arms and making subsequent choices. As before, rats would often pause at the edge of the center platform and orient toward the end of an arm before choosing it, and once the rat had all four legs on the arm, it almost always proceeded to the end before turning around.

All rats rapidly came to choose an average of more than seven different arms within the first eight choices. The mean numbers of different (i.e., correct) arms chosen within the first eight choices for the first 5 and second 5 days of this procedure were 5.7 (range: 4.2–6.6) and 7.6 (range: 7.2–8.0), respectively.

Rebait. Replacing food on one of the arms after the original piece of food was obtained had no obvious effect on the accuracy of choice behavior. Occasionally, the animals seemed to spend more time than usual sniffing and orienting after the food pellet was replaced, but this behavior was not reliable. No rat ever returned to the rebaited arm before making at least eight choices, and on 5 of the 30 tests (six rats, five tests each) the rebaited arm was never chosen within the 16-choice limit. The mean number of choices before returning to the rebaited arm was 11.6 (range: 9.6–13.0). Since the rats tended to choose all eight arms again during Choices 9–16, this performance is very similar to that expected by chance, indicating that the presence of food at the end of an arm had no observable effect on the accuracy of choice behavior.

Added odor. The addition of the after-shave lotion had a profound effect on the animals' general behavior. On the first day, all animals were greatly disturbed by the odor; some were very hesitant to run on the arms and spent most of the time on the center platform, while others would often go out to the end of an arm but refuse to pick up the pellet. By the second day, this behavior pattern was greatly diminished, and all animals ran readily down the arms and consumed all the pellets. In contrast to the disruption of general behavior, there was little change in the accuracy of choice behavior even on the first day. The mean number of correct choices within the first eight choices was 7.4 (range: 7.3–8.0) over all three days, indicating that the odor from food or some kind of odor trail had no observable effect on choice accuracy.

All procedures. To simplify the analysis of choice behavior during Choices 9–16, only those days on which the animals both (a) obtained all eight pieces of food within the first eight choices and (b) continued to make a total of 16 choices were analyzed. For the six rats, there was a mean of 9.2 (range: 3–11) days on which both Conditions a and b were met. On these days, the mean number of different arms chosen during the last eight choices was 5.9 (range: 4.3–7.2). Thus, even after all the food had been obtained from the apparatus, the rats still tended to choose each of the arms, although not to the extent as during Choices 1–8. A similar pattern of choice behavior was observed on days when Conditions a and b were not met.

The order of choices was analyzed to determine if the rats used specific sequences to simplify the problem. During all of the procedures, the rats almost never chose the same arm twice in succession and this sequence was found in less than .01% of the choices. In general, the rats tended not to choose adjacent arms, although occasionally there were sequences in which two, three, or four adjacent arms were chosen in order. The rats often responded in a clockwise or counterclockwise fashion and seemed to prefer the arm 90° from the arm just chosen. None of these patterns or any other pattern obvious to the experimenters was regularly exhibited by the rats, and, most importantly, choice accuracy was routinely high irrespective of the particular order of choices. Table A1 presents the raw data for Rat 3 during Days 10–27. Although various patterns of choices appear, there are no patterns that are a prerequisite for accurate choice behavior.

Discussion

In the typical sequential maze experiment, decision making at any given choice point can be independent of that at any other choice point. The rat must decide between only one pair of alternatives (i.e., go left, go right) at a time, and this choice can be made without reference to any preceding or subsequent choices. In the present experiment, however, the rat always returned to the center platform after each arm and was thus presented simultaneously with all the alternatives on each choice. Thus the rat cannot treat each successive choice independently. The paradigm may be summarized as one which requires sampling with replacement from a known set of items until the entire set is sampled.

Rats performed very accurately and within 5 days of free choice testing, all animals were consistently choosing an average of more than seven different arms within the first eight choices. The high level of accuracy even on the first 5 days of free choice testing suggests that the rats were predisposed not to repeat their choices in this situation, presumably reflecting the tendency toward spontaneous alternation commonly observed in two-choice paradigms in a simple T maze (Douglas, 1966). Accurate determination of "chance" performance levels given this alternation tendency is difficult. If each choice is completely independent of all others, the experiment becomes an example of the classic occupancy problem (Feller, 1950); the probabilities of choosing n different arms within the first eight choices are as follows: $n = 8$, $p = .0024$; $n = 7$, $p = .0673$; $n = 6$, $p = .3194$; $n = 5$, $p = .4200$; $n = 4$, $p = .1680$; $n = 3$, $p = .0168$; $n = 2$, $p = .0004$; $n = 1$, $p = 6 \times 10^{-8}$. The assumption of choice independence for rats is probably not valid because even on the first few days of exploration the rats almost never chose the same arm twice in succession and usually chose at least five different arms within eight choices. In another experiment (Olton, Note 1) infant rats were rewarded for a response to any arm, as in the typical reinforced spontaneous alternation paradigm. Under these conditions, the animals chose an average of six different arms within the first eight choices, indicating a strong disposition not to repeat choices to arms even when food was obtained on repeated arms as well as on new arms. The presence of a spontaneous alternation tendency does not compromise the basic argument about the discrimination requirements of the paradigm made above because in order to exhibit spontaneous alternation, the rat must obviously locate each arm and determine which arms have been chosen and which have not. The only difficulty imposed by the presence of spontaneous alternation tendency is accurate assessment of the relative contribution of learning and of innate behavioral tendencies to successful performance, and the present experiment was not designed to resolve this issue.

Rebaiting the arms with food and adding odor to the arms did not markedly influence the choice accuracy of the animals, demonstrating that cues such as the odor or sight of food at the end of an arm and odor trails from the animals themselves were unimportant in performing the task. A number of studies have demonstrated that odor can function as a cue for rats in spontaneous alternation (Douglas, 1966), maze learning (Means, Hardy, Gabriel, & Uphold, 1971;

Wasserman & Jensen, 1969) and other types of tasks (Slotnick, 1974). In all of the above experiments, however, the salience of the odor was markedly increased either by combining odors from many rats all tested under the same condition or by having all odors except the stimulus odor removed or suppressed. The data from the present experiment suggest that under more natural conditions where the odor trail from a single rat is deposited among the odor trails from many other rats exhibiting different behaviors, odor trails have a minimal effect on discrimination behavior.

The number of different arms chosen during Choices 9–16 was slightly less than that during Choices 1–8 but was still routinely high, suggesting that animals tended to choose each arm even after all the food had been obtained. The results of Experiment 6 to be reported below suggest that the slight decrease in the number of arms chosen during the second eight choices probably reflects the performance decrement that would be expected in the absence of differential reinforcement. The results offer further evidence for the unimportance of intramaze odor cues because after the first eight choices the rat would have left an odor trail on all arms, while there would be no food on the end of any arm; nonetheless, the number of different arms chosen was still high.

Rats have been shown to "chain" specific responses to form very long specific behavioral sequences (Hulse, Deese, & Egeth, 1975, p. 56). In the present experiment, however, no evidence of chaining was observed. Although the rats did occasionally repeat specific sequences of choices, these were not regularly observed and choice accuracy was routinely high irrespective of the particular choice pattern emitted. The data indicate that performance here is very different from that observed in behavior chains and that chaining was not utilized as a strategy to solve the task.

In summary, rats perform well in the present situation, choosing almost perfectly during their first eight choices. Intramaze odor cues and consistent sequences of choices are both unnecessary for successful performance.

EXPERIMENT 2

The present experiment was designed to provide additional evidence concerning the relative importance of intramaze cues, extramaze cues, and choice sequences. To this end, all arms were made equally wide so that arm width could not serve as a cue; animals were confined to the center platform by means of guillotine doors in order to interfere with any sequence of choices that might have been in process at this time; arms were interchanged so that all extramaze place cues were dissociated from all intramaze cues.

Method

Subjects and apparatus. The subjects were the same as in Experiment 1. The apparatus of Experiment 1 was modified in the following manner. All arms were 7 cm wide. A hole 2 cm wide and 1 cm deep was drilled at the end of each arm to serve as a food cup. The arms were made so that they could be detached from the center platform and interchanged with each other. A wooden compartment 10 cm high was constructed around the edge of the center platform. Wooden guillotine doors were placed in the walls of this compartment at the entrance to each arm so that the animal could be confined to the center platform. The top of the compartment was of Plexiglas.

Procedure

Free choice. For 10 days (Days 1–10) the rats were tested as in the free choice procedure of Experiment 1. They were allowed either 16 choices or 10 min. in the apparatus, whichever came first.

Confinement. For five days (Days 11–15) the rats were confined to the center compartment by lowering the guillotine doors when they entered the center compartment after their third choice. The doors remained lowered for approximately 1 min., after which they were raised and the rat was allowed to choose freely until a total of 16 choices had been made or 10 min. elapsed.

Interchange arms. For 15 days (Days 16–30) the rats continued to be confined to the center compartment after their third choice. During this confinement, two adjacent arms were interchanged. The actual arms moved varied each day, but in all cases one arm had been chosen during the first three choices and the other arm had not, placing spatial location cues and arm cues in opposition to each other. In the following discussion, "chosen arm" refers to the arm of the interchanged pair which had been chosen prior to the arms being interchanged, whereas "unchosen arm" designates the arm of the interchanged pair which had not been chosen prior to the arms being interchanged. Likewise, "chosen spatial location" refers to the position in the testing room of the arm of the interchanged pair which had been chosen prior to the arms being interchanged, and "unchosen spatial location" indicates the position in the testing room of the arm of the interchanged pair which had not been chosen prior to the arms being interchanged. To the extent that arm cues (such as odor trail) direct choice behavior, the rats should go to the unchosen arm while repeating their choice of spatial location. To the extent that spatial location cues direct choice behavior, the rats should go to the unchosen spatial location while repeating their choice of the arm. Three rats were reinforced for responding to the unchosen arm following confinement, while the other three rats were reinforced for responding to the unchosen spatial location following confinement. Reinforcement on the remaining six arms was not altered.

Results

Free choice. Making all arms the same width had no obvious effect on the accuracy of choice behavior. The mean number of different arms chosen during the first eight choices was 7.7 (range: 7.4–7.9).

Confinement. While confined to the center compartment the animals were usually active, exploring and moving about the compartment for the duration of the confinement period. When the guillotine doors were opened, the rat usually approached and oriented toward the end of the arm it was facing. The accuracy of choice behavior was not at all affected by the confinement; the mean number of different arms chosen in the first eight choices was 7.7 (range: 7.0–8.0).

Interchange arms. In all cases, the animals showed little, if any, evidence of reaction to the interchanged arms and continued to choose almost entirely on the basis of spatial location. The rats reinforced for responding to the unchosen arm performed no differently from the rats reinforced for responding to the unchosen spatial location and the data from both groups were combined for analysis. In all but 2 of the 90 tests (6 rats, 15 tests each), rats responded to the unchosen spatial location prior to responding to the unchosen arm. The mean choice number of the unchosen spatial location was 6.1 (range: 4.1–7.3) and was thus always among the first eight choices when food was usually obtained, while the mean choice number of the unchosen arm was 9.9 (range: 8.5–11.7) and thus always among the second eight choices when food was usually not obtained. The accuracy of the first eight choices for spatial location did not decrease as a result of interchanging arms, and the mean number of different spatial locations chosen within the first eight choices was 7.7 (range: 7.6–7.8).

All procedures. The order of choices was again analyzed. As in the previous experiment, various patterns often appeared on a particular day, but none of these patterns was necessary for accurate choice behavior. The rats also tended to choose each of the eight different arms on Choices 9–16.

Discussion

The results of Experiment 2 extend and corroborate those of Experiment 1 describing the types of cues directing the rats' choice behavior. First, the unimportance of intramaze cues was supported by continued high performance when all arms were changed to equal width. Second, the unimportance of specific continuous responses sequences was supported by continued high performance when animals were confined to the center compartment between choices. Third, the unimportance of intramaze odor cues was strongly corroborated by interchanging the arms. Those animals reinforced for responding to the unchosen spatial location were unaffected by the presence of an odor trail and those animals reinforced for responding to an odor trail (i.e., the unchosen arm) showed no evidence of learning to follow the odor trail.

EXPERIMENT 3

Experiment 3 was designed to replicate and extend the results of the previous experiments in three ways. First, the apparatus was altered so that instead of interchanging just two arms and leaving any possible intramaze cues on the center platform unaltered, the entire arm assembly could be rotated while the animal was in the center compartment, and then the entire center compartment rotated while the animal was on an arm, thus rotating all intramaze cues instead of just those on two arms. Second, no exploration period took place prior to training to obtain a better estimate of the rate of learning. Third, new animals were used to determine the reliability of the previous findings.

Method

Subjects. The subjects were six experimentally naive male albino rats that weighed 300–325 g at the start of testing. All rats were food deprived to 85% of ad-lib body weight and were maintained at this level throughout the experiment by feeding the required amount of Purina Lab Chow within ½ hr. after testing.

Apparatus. The apparatus was the same as in Experiment 2 except that the arms and center compartment were suspended independently of each other so that each could be rotated without disturbing the other. Reinforcement consisted of one 190-mg Noyes food pellet at the end of each arm.

Procedure

Free choice. For the first 15 days, each rat was given one daily test session until 16 choices were made or a total of 10 min. had elapsed. On the first few days, rats were shaped as necessary to go onto the arms by placing food reinforcement close to the center platform. On Days 6–15, all food was placed in the food cups at the end of the arms.

Confinement. For the next five days (Days 16–20), each rat was confined in the center compartment for approximately 1 min. after the third choice by lowering the guillotine doors after the rat entered the center compartment.

Maze rotation. For the next six days (Days 21–26), each rat was confined to the center compartment after the third choice as described above. On Days 1, 3, and 5, while the rat was confined to the center compartment which did not move at this time, the arm assembly was rotated 45° and food was replaced on the three arms already chosen on that day so that all arms contained food. The guillotine doors were then raised and, while the rat was on the arm making its fourth choice, the center compartment was rotated 45° in the same direction as the arm assembly. Thus, all intramaze cues were rotated 45° with respect to extramaze cues. The rat was allowed to choose freely until either the five remaining arms or the five remaining spatial locations were chosen. On Days 2, 4, and 6, each rat was confined after the third choice and the maze arms were rotated, but the arms were returned to their original location before the guillotine doors were raised in order to control for any general influence of the movement of the apparatus on choice behavior.

Results

Free choice. For the first few days, all rats were reluctant to enter the arms and spent considerable time exploring the center compartment and orienting toward the end of the arms. Within 8 days, all animals readily ran on the arms, retrieving and consuming all the food pellets. Their general behavior was similar to that reported previously; the rats often hesitated at a guillotine door and oriented toward the end of the arm. Once all four legs were placed on the arm, however, the rat almost always ran to the end of the arm and placed its nose in the food cut. No data are reported for the first 5 days because the rats often did not leave the center compartment to run down the arms. As soon as the rats began to run and choose all the arms, they very rapidly demonstrated the discrimination; the mean numbers of different arms chosen within the first eight choices for Days 6–10 and Days 11–15 were 7.2 (range: 6.8–7.4) and 7.3 (range: 6.8–7.8), respectively.

Confinement. Confinement had no obvious effect on the animals' behavior. As before, the animals were active during the period of confinement, exploring the center compartment; when the guillotine doors were open, they usually oriented down the arm they were facing. The mean number of different arms chosen within the first eight choices during this period was 7.6 (range: 7.5–7.8).

Maze rotation. To the extent that intramaze cues control choice behavior, the rats ought to go to the five remaining unchosen arms after maze rotation and repeat choices to already chosen spatial locations. To the extent that extramaze cues control choice behavior, the rats ought to go to the five remaining unchosen spatial locations after the maze rotation and repeat choices to already chosen arms. Consequently, the relevant data are the number of times an already chosen arm was repeated after the maze rotation and the number of times an already chosen spatial location was repeated after the maze rotation. Since three choices were made prior to the maze rotation, the maximum number of repetitions was three. The mean number of repetitions of already chosen arms was 2.3 (range 1.3–2.7), while the mean number of repetitions of already chosen spatial locations was .3 (range: .0–1.0). Additionally, the mean number of different spatial locations chosen within the first eight choices was 7.5 (range: 7.2–8.0) on days when the maze was rotated, the same values as on control days.

Discussion

Results of Experiment 3 are consistent with those of the previous two experiments. After the animals learned to run out onto the arms, their choice accuracy was very high. Manipulation of intramaze cues appeared to have no effect on choice behavior; all animals continued to choose accurately on the basis of spatial location and readily repeated already chosen arms in order to respond to unchosen spatial locations.

EXPERIMENT 4

The results of Experiments 1, 2, and 3 consistently demonstrate that the rats did not choose adjacent arms, did not rely on any type of intramaze marking, did not chain responses in particular sequences, and did not utilize any other obvious strategy to simplify the task. Since the testing procedure involved sampling with replacement, the rats must have been able to identify each of the eight arms and remember over a period of several minutes which arms had been chosen and which had not. In short, the rats seemed to treat each of the eight spatial locations separately.

The purpose of the present experiment was to investigate the characteristics of the memory for this list of items. In particular, the following questions were addressed: (a) Is there a performance decrement as the number of choices the animals makes increases? (b) Are correct choices at the beginning of the test remembered better than subsequent choices? (c) Are correct choices at the end of the test remembered better than previous choices? (d) Is there spatial generalization from one arm to the other so that errors tend to be directed towards correct choices? (e) Is information from correct choices and incorrect choices stored in the same or different memory stores?

Method

Subjects and apparatus. The subjects were 12 experimentally naive male albino rats that weighed 300–325 g at the start of testing. The apparatus was the same as in Experiment 3.

Procedure

Rats were placed on a food deprivation schedule to bring body weights down to 85% of ad lib; daily feeding took place approximately ½ hr. after testing. Reinforcement was a 45-mg Noyes pellet. At the start of each test session, one pellet was placed at the end of each arm; reinforcement was not replaced during the test so that a maximum of eight pieces of food could be obtained. Each rat was placed on the center platform of the apparatus and allowed to make choices until all eight reinforcements were obtained or until 10 min. had passed, whichever came first. All rats were tested for 30 days, with one test on each day.

Data Analysis

The data analysis for this experiment will be presented in considerable detail because there are a number of different factors that must be considered and there are no readily available precedents to use as a model for analysis. For each question, two types of analyses will be presented. The first is designed to describe the actual performance of the animal and will provide information about the observed behaviors. The second is designed to adjust for various influences which must be considered in order to adequately answer the five questions posed in the introduction. In all cases, the procedure chosen for the second analysis is the one that seems most appropriate to the question being asked. But for any set of raw data and any particular experimental question, a number of different analyses are possible, each based on a different set of assumptions and with its own set of advantages and disadvantages (e.g., see Murdock, 1957). Thus, the first analysis not only provides information about the way in which the animals actually performed but also allows the reader to carry out other transformations of the data if desired.

All but one of the analyses are based on errors (i.e., returning to an already chosen arm). As in the previous experiments, the animals performed well and made relatively few errors. In order to obtain enough observations for an accurate estimate of behavior, the data for each animal were averaged over several days. Since choice accuracy gradually improved during the first 10 days and then remained stable during the second 10 days, the data were analyzed in 10-day blocks. Thus, the results from the first 10 days probably reflect some learning of the task, while the results from the second 10 days probably reflect asymptotic performance of the already learned task. During Days 11–20, one animal performed perfectly so that there were no errors to analyze. Four other animals had either one or two errors, whereas the remaining seven had four or more errors. For Days 11–20, the data were analyzed separately for $n = 11$ and for $n = 7$.

Analyses of variance were for a treatments by subjects design.

Results

A mean of 4.9 days (range: 3–8) was required before a total of eight choices were made within the 10-min. test limit. On subsequent days, animals chose readily, always completing at least eight choices and usually finishing the day's test within 2 min. For purposes of data analysis, Day 1 was defined as the first day on which at least eight choices were made.

Choice accuracy: General. The task was learned rapidly and even during Days 1–5 the mean number of different arms chosen within the first eight choices was 6.9 (range: 6.0–7.3). Performance improved to a mean of 7.5 (range 6.9–7.8) correct responses during Days 6–10 and then appeared to reach an asymptote at about 7.6 (range: 7.3–8.0) correct responses within the first eight choices on Days 11–15 and Days 16–20. All but one animal performed better during Days 16–20 than during Days 1–5, indicating that although initial performance was accurate, a significant amount of learning did take place.

Performance decrement: Observed probability of a correct response. Performance accuracy was greatest at the beginning of each day's test and declined slightly but consistently during the test. Table 1 presents the means for the observed probability of a correct response, $p(\text{cor})_{\text{obs}}$, on Choices 2–8 during Days 1–10 and Days 11–20. Data for the first choice are not presented because the first response had to be correct. Data for choices following Choice 8 are not presented because the animals often performed perfectly and testing was terminated at the end of 8 choices. The observed probability of a correct response was determined for each animal for each choice by the formula

$$p(\text{cor})_{\text{obs}} = \frac{\text{number of correct responses}}{\text{total number of responses}} \times 100.$$

The $p(\text{cor})_{\text{obs}}$ score can range from 100, which indicates a correct response on all choices, to 0, which indicates an incorrect response on all choices. As would be expected from the data reported in the previous paragraph, $p(\text{cor})_{\text{obs}}$ was high on all choices. Scores were greatest on the second choice and steadily declined until the eighth choice (Days 1–10, $F(6, 66) = 5.1$, $p < .01$; Days 11–20, $F(6, 66) = 9.6$, $p < .01$). Scores for all choices were greater during Days 11–20 than during Days 1–10, reflecting the improvement in accuracy reported in the previous paragraph as measured by the number of correct responses within the first 8 choices.

TABLE 1

PROBABILITY OF A CORRECT RESPONSE

Probability	Choice						
	2	3	4	5	6	7	8
Observed							
Days 1–10	100	95	95	85	87	81	78
Days 11–20	100	99	98	96	97	90	80
Transformed							
Days 1–10	100	84	86	71	77	70	73
Days 11–20	100	96	95	91	95	88	77

Performance decrement: Transformed data. Although the observed probabilities in Table 1 are clear, there is a bias present which tends to exaggerate the magnitude of the decline in performance during the test. This bias arises because the chance probability of a correct response decreases as the number of arms already chosen increases. For example, consider a sequence of choices in which all choices are correct; on the second choice, seven of the eight arms still contain food, on the third choice, six of the arms contain food, and so on. Thus any given $p(\text{cor})_{\text{obs}}$ score may reflect relatively better performance after many choices than it does after few choices, and the observed probability data tend to exaggerate the decline in performance that occurs during each test. To correct for this bias, the changing expected chance probability of a correct response must be considered. To this end, $p(\text{cor})_{\text{exp}}$ was defined as the expected chance probability of a correct response on any given choice according to the formula

$$p(\text{cor})_{\text{exp}} = \frac{\text{number of arms not chosen}}{8} \times 100.$$

The observed probability data from Table 2 were then transformed for each animal for each choice, according to the formula below, to provide a measure of the extent to which the animals exceeded chance performance and attained maximum performance on each choice:

$$\frac{\mathrm{p(cor)_{obs}} - \mathrm{p(cor)_{exp}}}{100 - \mathrm{p(cor)_{exp}}} \times 100.$$

This formula corrects for the expected chance performance on each choice and expresses performance as a proportion of the maximum performance obtainable, allowing performance on any given choice to be directly compared with performance on any other choice. The transformed scores can range from 100, indicating the maximum number of correct responses, through 0, indicating random performance with $\mathrm{p(cor)_{obs}} = \mathrm{p(cor)_{exp}}$, to -100, indicating the maximum number of incorrect responses. [When $\mathrm{p(cor)_{obs}}$ was less than $\mathrm{p(cor)_{exp}}$, the denominator was changed to $\mathrm{p(cor)_{exp}} - 0$.] For example, consider an animal with $\mathrm{p(cor)_{obs}} = 80$. If this performance was on the third choice and six arms had not been chosen, the transformed score would be $(80 - 75)/(100 - 75) \times 100 = 20$. If this performance was on the seventh choice and two arms had not been chosen, the transformed score would be $(80 - 25)/(100 - 25) \times 100 = 73$. As the number of correct arms available to the animal decreases, the transformed score for any given $\mathrm{p(cor)_{obs}}$ increases. Also, as the accuracy of performance increases, the transformed score approaches the asymptote of 100.

Table 1 also presents the transformed scores. An important result is that all scores were substantially greater than 0, indicating greater than chance performance on even the last choice. The same trends that were apparent in the observed probabilities are also apparent here. First, performance gradually decreased as the number of choices increased (Days 1–10, $F(6, 66) = 6.5$, $p < .01$; Days 11–20, $F(6, 66) = 7.1$, $p < .01$). Second, performance during Days 11–20 was better than performance during Days 1–10.

Sequential position of errors: General analysis. The data in Table 1 indicate that the likelihood of making an error increased with the number of choices made by the animal, but they do not provide information about the sequential characteristics of these errors. When errors did occur, they were almost always to arms that had been chosen at the beginning of the day's test and rarely to arms chosen near the end of the day's test. Four different analyses of these data were performed to provide a quantitative measure of this behavior: (a) the observed relative probability of repeating each of the first seven correct choices, $\mathrm{p(rep)_{obs}}$; (b) correction of $\mathrm{p(rep)_{obs}}$ for the differing number of opportunities to repeat each choice; (c) correction of $\mathrm{p(rep)_{obs}}$ for the possibility of an arm preference; (d) correction of $\mathrm{p(rep)_{obs}}$ for both differing number of opportunities to repeat each choice and the possibility of an arm preference.

In all of these analyses, the results were the same. The probability of an error was greater for arms chosen early during each day's test than for arms chosen later during each day's test (for Days 1–10 and Days 11–20, $ns = 7$ and 11, respectively). Analyses of variance yielded Fs ranging from 2.7 to 15.9 ($p < .05$ for 2 tests, $p < .01$ for 10 tests). To conserve space, each analysis is described, but only the data from Days 1–10 are presented in Table 2. Table A2 presents the raw choice data upon which the analyses are based.

Sequential position of errors: Observed relative probability of repetition. The observed relative probability of repeating each of the first seven correct choices was determined. The eighth choice was not considered because testing was terminated after this choice and there was no opportunity to repeat the eighth choice. For each animal for each day, all correct choices were ordered in sequence from one to seven. Then for each animal, the observed relative probability of repeating each correct choice was calculated according to the formula

$$\mathrm{p(rep)_{obs}} = \frac{\text{number of repetitions of choice}}{\text{total number of repetitions}} \times 100.$$

The $\mathrm{p(rep)_{obs}}$ score indicates the relative probability of an error to each of the previous

TABLE 2

Relative Probability of Repeating a Previously Correct Choice: Days 1–10

Analysis	Correct choice 1	2	3	4	5	6	7
Observed probability	36	23	15	12	8	6	1
Correction for opportunities	43	30	23	23	19	21	5
Correction for arm preference	31	23	17	12	9	6	.8
Correction for opportunities and arm preference	40	31	27	26	22	25	8

correct choices. Scores can range from 100, indicating that all errors were made by repeating one particular choice, to 0, indicating that no errors were made by repeating that choice. The sum of scores for all choices equals 100. Consider an animal that made a total of 8 errors in 10 days. If 5 of these errors were to the first correct choice, 2 to the second correct choice, and 1 to the third correct choice, then the $p(\text{rep})_{\text{obs}}$ score for correct Choices 1, 2, and 3 would be 63, 25, and 13, respectively. The first line of Table 2 presents the mean of $p(\text{rep})_{\text{obs}}$ scores for Choices 1–7 during Days 1–10. As can be seen from Table 2, when errors occurred, they were most likely to be to the first correct choice, and there was a steady decline in the probability of repetitions for each subsequent choice.

Sequential position of errors: Adjustment for opportunities. There is a bias in the data, as presented in the first line of Table 2, because the number of opportunities available for repeating each choice differs. The animals made an average of 9.3 choices to obtain all eight pieces of food during Days 1–10 and, thus, had an average of 8.3 opportunities to repeat the first correct choice and a maximum of 7.3 opportunities to repeat the second choice, 6.3 for the third, and so on. An adjusted score, correcting for the number of opportunities to repeat each choice, was calculated for each animal for each choice according to the formula

$$\frac{p(\text{rep})_{\text{obs}}}{\text{number of choices remaining}} \times 100.$$

Unlike $p(\text{rep})_{\text{obs}}$, these adjusted scores do not sum to 100 and range from 0 to greater than 100. The adjusted scores can be 0, indicating that no errors were made to the choice in question, or they can be some positive number, indicating that some errors were made to that choice: The greater the number, the greater the number of errors made to that choice. These adjusted scores also correct for the number of opportunities to repeat each choice and indicate the likelihood of repeating a choice any time an error is made. Consider an animal with $p(\text{rep})_{\text{obs}} = 40$ for the first 10 days. If this score was obtained for the first choice of each day and there was a total of 80 choices following the first choice (indicating that the animal obtained all eight pieces of food in an average of 9.0 trials), then the adjusted score would be $40/80 \times 100 = 50$. If this score was obtained for the third choice of each day and there were 50 choices remaining (indicating perfect performance on Choices 2 and 3), then the adjusted score would be $40/50 \times 100 = 80$. For any given $p(\text{rep})_{\text{obs}}$ score, the adjusted score increases as the number of choices remaining decreases. The second line of Table 2 presents these adjusted scores.

Sequential position of errors: Adjustment for arm preference. If a strong preference for one or more arms developed, the animal might tend to choose this arm early during each day's test. If the arm preference were constant throughout the day's test, then the animal might continue to have a strong tendency to respond to this arm. Such a combination of events would lead to the animal repeating early choices, not because of the position of the choice within the sequence but rather because of a preference for the arm. An attempt to evaluate and correct for any possible arm preference was undertaken by determining for each arm the average sequential position of the first correct response to that arm on each day. Each first choice of an arm was given a score of 1, each second choice, 2, and so on. All these scores were summed and divided by 10 for each 10-day block. The resulting sequential position score provides for each arm the average sequential position of the first correct response to that arm during the 10 days. For each animal, the scores must total 36 (i.e., $1 + 2 + \ldots + 8$). The expected chance performance score for any arm is 4.5 (i.e., $[1 + 2 + \ldots + 8]/8$). A score of less than 4.5 indicates a tendency to choose an arm preferentially early in the day's test; a score of more than 4.5 indicates a tendency to choose an arm preferentially late in the day's test. All animals exhibited a preference for some arms over others. The mean (and standard error of the mean) sequential position score of the most preferred arm was 2.8 (± .06) and of the least preferred arm was 6.0 (± .04). In order to determine how stable any arm preference might be, a correlation coefficient was calculated for each animal between the preference scores on each of the eight arms for Days 1–10 and the preference scores on each of the eight arms on Days 11–20. The correlations ranged from −.63 to .89, with an average of .46.

The $p(\text{rep})_{\text{obs}}$ scores of the first line of Table 2 were adjusted for arm preference by using sequential position as a weighing factor, multiplying each arm by its sequential position score, according to the formula

$$\frac{\text{sequential position score for choice}}{\text{sum of sequential position scores for all choices}} \times 100.$$

Like $p(\text{rep})_{\text{obs}}$, the adjusted scores can range from 100, indicating that all errors were made by repeating that particular choice, to 0, indicating that no errors were made by repeating that particular choice, and the sum of scores for all choices is 100. Unlike $p(\text{rep})_{\text{obs}}$, the adjusted scores correct for the relative preference for each arm as measured by the sequential position score. Consider an animal that repeats choices to Arms 1, 4, and 5 which have sequential position scores of 1.5, 4.0, and 4.5, respectively, and makes two repetitions of the first choice, both of Arm 1, and two repetitions of the second choice, one each to Arms 4 and 5. The adjusted score for the first choice is $[(1.5 + 1.5)/11.5] \times 100 = 27$, the adjusted score for the second choice is $[(4.0 + 4.5)/11.5] \times 100 = 86$, and the adjusted scores for all other choices is 0. [The unadjusted $p(\text{rep})_{\text{obs}}$ scores reported as in Table 1 would be: Choice 1, $2/4 \times 100 = 50$; Choice 2, $2/4 \times 100 = 50$; all other choices = 0.] Thus for any given $p(\text{rep})_{\text{obs}}$ score, the greater the preference for an arm as measured by the sequential position score, the lower will be the adjusted score. Line 3 of Table 2 presents these adjusted scores.

Sequential position of errors: Adjustment for arm preference and opportunities. The data in the third line of Table 2 were adjusted to correct for the number of choices remaining following each correct choice according to the formula

$$\frac{\left[\dfrac{\text{sequential position score for choice}}{\text{sum of sequential position scores for all choices}} \times 100\right]}{\text{number of choices remaining}} \times 100.$$

The adjusted scores can be 0, indicating that no errors were made to the choice in question, or they can be some positive number, indicating that some errors were made to that choice; the greater the number, the greater the relative number of errors made to that choice. These adjusted scores, which are presented in the fourth line of Table 2, correct for both arm preference and the number of opportunities to repeat each choice and indicate the likelihood of repeating a choice any time an error is made.

Generalization among arms. Table 2 presented information about the sequential characteristics of errors but did not provide information about their spatial characteristics. When errors occurred, they appeared to be randomly distributed among the 8 arms and not preferentially directed near correct arms. An analysis of the spatial distribution of each error was conducted by numbering the position of the arm on which the error occurred as 0, the position of the arms at 45° (immediately adjacent), as 1, the position of the arms at 90° as 2, the position of the arms at 135° as 3, and the position of the remaining arm at 180° as 4. An error location score, indicating the average distance between the repeated arm and each of the remaining correct arms was calculated from the formula

$$\frac{\text{sum of positions of remaining correct arms}}{\text{number of remaining correct arms}}.$$

Error location scores can range from 1.0, indicating that an error was always made to an arm adjacent to a correct arm, to 4.0, indicating that an error was always made to the arm farthest away from a correct arm. Since there were two arms each at 45°, 90°, and 135°, which were assigned values of 1, 2, and 3, respectively, and one arm at 180°, which was assigned a value of 4, chance performance would be $(2 + 4 + 6 + 4)/7 = 2.29$. The error location scores for Days 1–10 and Days 11–20 ($n = 7$ and $N = 11$) ranged from 2.3 ($\pm .03$) to 2.5 ($\pm .1$). These scores closely approximate the values expected by chance, indicating that when errors were made, there was no tendency for them to be to arms close to correct ones.

Effect of errors on choice accuracy. Table 1 demonstrated that there was a general performance decrement as the number of choices increased within a day but did not consider whether the previous choices were correct or incorrect. A correct choice was, by definition, a new choice for the animal, one which had not been made previously on that day. An incorrect choice was a repetition of an earlier choice, and thus the animal made two responses to the same arm. If the rat treats the repetition of an arm as if it were any other choice, then there ought to be an additional performance decrement as a result of the additional choice, reflecting the results reported in Table 1. If the rat treats the repetition of the arm as if it were another instance of the original choice, then there ought to be no additional performance decrement as a result of the error. The question is whether the rat processes the error as a new piece of information, which would result in a performance decrement on subsequent choices or as confirmation of an already acquired piece of information, which might not result in a performance decrement. To answer this question, an analysis was conducted to determine the probability of a correct response on Choice 7 and Choice 8 as a function of the number of prior errors. For each rat, test days were grouped according to the number of errors made prior to Choice 7 for the analysis of choice accuracy on Choice 7 and the number of errors prior to Choice 8 for the analysis of choice accuracy on Choice 8. Only Choices 7 and 8 were analyzed because the frequency of errors prior to other choices was too low to provide a sufficient number of observations for analysis. The probability of a correct response was then determined by the formula

$$\frac{\text{number of correct responses}}{\text{number of observations}}.$$

Scores can range from 0, indicating that none of the observed responses were correct, to 100, indicating that all of the observed responses were correct. As in Table 1, these scores are biased because the chance probability of a correct response varies according to the choice number and the number of previous errors. Consequently, the same correction as applied in Line 2 of Table 1, was carried out here according to the formula

$$\frac{\dfrac{\text{number of correct responses}}{\text{number of observations}} - p(\text{cor})_{\text{exp}}}{1 - p(\text{cor})_{\text{exp}}}.$$

These scores have the same characteristics as those presented in the second line of Table 1.

The results of both analyses indicated that there was no systematic change in the probability of a correct response as a function of the number of preceding errors. The observed trend was in all cases for the probability of a correct response to be lower after an error than after all correct choices, but this trend was not reliable.

Discussion

The results of Experiment 4 provide evidence about the characteristics of the memory processes the rats use while solving the problem. First, the data indicate that there was a progressive decrease in choice accuracy as the number of choices increased (Table 1, Line 1), even when a correction was made for the expected chance level (Table 1, Line 2). Second, when errors occurred, there was no tendency for them not to be made to the arms chosen on the first few choices of the day (Table 2, Line 1), even when corrections were made for the number of opportunities to repeat choices (Table 2, Line 2), arm preference (Table 2, Line 3), and both the number of opportunities to repeat choices and arm preferences (Table 2, Line 4).

Third, when errors occurred, there was a reliable tendency for them not to be made to the arms that had just been chosen (Table 2, Line 1), even when the corrections indicated above had been taken into consideration (Table 2, Lines 2–4), although this tendency was relatively weak. Fourth, there was no indication that the performance decrement as a function of increasing numbers of choices was lessened by an error; indeed, the trend that appeared (which was statistically insignificant) was for an increase in the performance decrement (i.e., a lowered probability of a correct response) after an error. Fifth, there was no indication that the animals confused the different spatial locations and directed their errors toward arms that were adjacent to correct arms.

Two types of corrections were made on the observed data. The first considered the changing chance probability of a correct response and was straightforward. The second considered the possibility of a preference for particular arms, and the assumption was made in Table 2, Lines 3 and 4, that an arm preference did exist. The low correlation between arm preference for Days 1–10 and Days 11–20 raises a question as to the validity of this assumption. One possibility is that an arm preference did exist but was unstable and changed markedly from one block of days to the next. An alternative is that an arm preference did not exist but resulted from the limited sample size which was available for analysis. In any case, the correction for arm preference changes the data in only a quantitative and not a qualitative manner so that the conclusions from this experiment remain unaltered. The absolute magnitude of the tendency not to make errors to recently made choices must remain in question however.

The finding that rats did not tend to make errors to arms close to correct arms suggests that there was no generalization gradient among the arms and that each arm was treated independently of the others. These results appeared to be contradictory to those of Tolman et al. (1946), who reported that in a "sunburst" maze, rats tended to choose arms close to the one that led to the goal. In that experiment, however, passage down the correct arm was blocked. In the present experiment, of course, access to all arms was available at all times. Thus, the paradigms are substantially different, and the results cannot be directly compared.

EXPERIMENT 5

Experiment 4 demonstrated that there was a performance decrement as the number of choices increased (see Table 1) but left unresolved the issue of whether this decrement was due to the number of choices or just the passage of time. Experiment 5 was designed to answer this question by forcing the animals to spend time in the center compartment between the fourth and fifth choices. If the performance decrement in Experiment 4 was due simply to the passage of time, then performance on Choices 5–8 should be impaired after confinement. If the performance decrement was due to the choices themselves, then no impairment should be observed.

Method

Subjects and apparatus. The subjects were six of the animals from Experiment 4 maintained under the same conditions. The apparatus was the same as in Experiment 4.

Procedure

The procedure was the same as in Experiment 4 except that animals were confined to the center platform after the fourth choice. One test was given each day for 5 days. When the animal returned to the center platform following the fourth choice, the guillotine doors were lowered for at least 2 min., more than the amount of time that the animals usually required to complete the last four choices. Following this confinement, the guillotine doors were raised and the animals allowed to choose freely until the remaining four reinforcements were obtained.

Results

All animals readily adapted to the confinement procedure. During the confinement period, the animals were active and moved about the center compartment in the typically exploratory fashion of rats. When the guillotine doors were raised, the rats usually proceeded to the nearest arm and either chose that arm or moved to some other arm. There was no obvious change in the accuracy of performance as a result of the confinement procedure, and the mean of the number of different arms chosen within the first 8 choices was 7.5 (range: 7.2–7.9) as compared with 7.6 (range: 7.3–8.0) during Days 11–20 of the previous experiment.

Discussion

Interference theories of forgetting propose that information is lost because new information interferes with the storage or retrieval of previous information and not simply because of the passage of time (Hulse et al., 1975). The current evidence supports the notion that the decrement in performance observed here occurs because of the increased number of choices and not because of the time interval between previous choices and the current choice. The period over which rats can successfully remember their previous choices was not addressed by the present experiment, but is obviously much longer than the few minutes tested here and may approach the limits found in spontaneous alternation experiments (Douglas, 1966; Walker, 1956).

EXPERIMENT 6

The results of Experiment 4 demonstrated that there was a performance decrement as the number of choices increased on each day but that this decrement was alleviated between days. These results indicate that the animals were able to successfully separate each day's performance from the previous day's performance so that choice accuracy recovered to its original level at the start of each day. The present experiment was designed to determine how rapidly recovery of performance could take place. To this end, the rats were allowed to choose all eight arms and were then confined to the center platform while all eight arms were rebaited. They were then allowed to choose all eight arms again. The results suggest that the animal could successfully "parse" their learning under these conditions so that there was almost complete recovery of choice accuracy between the last choice of the preceding series and the first choice of the following series.

Method

Subjects and apparatus. The subjects were four rats of Experiment 4 maintained under the same conditions as described previously. The apparatus was the same as in Experiment 4.

Procedure

The procedure was the same as in Experiment 4 with the following exception: After completing choices of each of the eight arms, the animals were confined to the center platform by the guillotine doors. All arms were rebaited, a process which required about 1 min., the guillotine doors raised, and the animals allowed to choose freely until all eight arms were chosen again. This procedure was then repeated until the animal was given a total of four tests on each of the first 5 days and a total of eight tests on each of the next 5 days.

Results and Discussion

On the first day of repeated tests, the animals chose normally on the first test but tended to choose slowly on the subsequent tests. On the following days, the animals readily adapted to the procedure and chose at their normal rate on all tests. There was a slight but consistent decrease in choice accuracy from the level demonstrated on the first test of the day, but even on the first day that the animals were given four tests, choice accuracy on the last choice was still greater than that observed on the first 5 days of Experiment 4. The same results were found when the animals were given eight tests on each day, demonstrating that the rats were able to successfully separate each test in memory so that the performance decrement expected as a function of the increasing number of choices did not appear. Table 3 presents these data.

GENERAL DISCUSSION

The general testing paradigm described here may be summarized as sampling with replacement from a known set of items until the entire set is sampled. The cognitive requirements of the present paradigm are straightforward; the animal must be able to locate each of the eight different arms and determine whether each arm has been chosen. Numerous strategies are possible to solve the task. The easiest approach would be to

TABLE 3

Performance on Consecutive Tests

Test	Four tests				Eight tests			
	First day		All days		First day		All days	
	M	*SEM*	*M*	*SEM*	*M*	*SEM*	*M*	*SEM*
1	7.3	.3	7.8	.1	7.8	.1	7.9	.04
2	7.3	.2	7.4	.3	8.0	.0	7.6	.1
3	6.8	.4	6.9	.1	7.5	.2	7.5	.03
4	6.8	.3	6.9	.1	7.3	.4	7.5	.0
5					6.8	.4	7.3	.2
6					7.0	.3	7.5	.2
7					7.5	.2	7.4	.2
8					7.5	.2	7.3	.1

Note. Table entries are the mean and standard error of the mean of the number of correct choices with the first eight choices of each test.

choose adjacent arms; the choice strategy could be stored in long-term memory, and the arms could be responded to as they appeared and would not have to be individually identified or remembered. A slightly more complicated approach would be to mark each arm as it is chosen; the marking system could be stored in long-term memory and choices guided by the number of marks present at each arm. Many other types of strategies are available as well. But the data indicate that the rats did not choose adjacent arms, did not rely on any type of intramaze marking, did not chain responses in particular sequences, and did not utilize any other obvious strategy to simplify the task. Since the testing procedure involves sampling with replacement, the rats must have been able simultaneously to identify each of the eight arms and remember over a period of several minutes which arms had been chosen and which had not.

Further analysis of the cues used by rats was not undertaken in the present experiment because it was not relevant to determining the general type of strategy used in solving the problem. Previous research has indicated that rats in maze tasks utilize almost any cue available to them (see discussion by Kimble, 1961; Woodworth, 1938). Such is probably also the case here, but accurate evaluation of the relative importance of different extramaze, directional, or compass cues requires a major experimental effort and is beyond the scope of the present paper.

Other experiments, although slightly different from the present one, have used a procedure closely resembling sampling with replacement from a known set of items until the entire set is sampled. The most similar animal experiment is that of Menzel (1973). Monkeys first observed 18 food items being distributed around a large enclosed compound and were then released to search for the food. They obtained an average of 12.5 food items and did so in a manner which minimized the distance which had to be traveled. The most similar human experiment is the "missing scan" technique of Buschke (1963). Subjects were successively presented with all but one item from a known list and then asked to identify the item that was missing. They had no difficulty in performing accurately with lists of up to 12 items. Thus, rats, monkeys, and humans all demonstrate a substantial memory capacity when tested in a sampling with replacement paradigm. An interspecies comparison of the limits of the memory capacity is not yet possible because of differences in procedure among the experiments and because none of the experiments was difficult enough to substantially challenge the subjects' capabilities. Other experiments (Dennis, 1939; Hunter, as analyzed by Miller & Frick, 1949; Tinkelpaugh, 1932; Yerkes & Yerkes, 1928) have also addressed the issue of memory for multiple events, but the paradigms were different enough from the present one to prevent direct comparison of results.

The rats rapidly attained almost perfect performance. Even performance on the first few days was far above the level expected on the basis of mathematical chance. But mathematical chance assumes that all choices are independent of each other—an assumption which is undoubtedly invalid. Other experiments (Olton, Note 1) have demonstrated a substantial spontaneous alternation tendency (Douglas, 1966) in the present situation; when all choices of all arms were reinforced, rats still chose an average of approximately six different arms within eight choices, a performance level which is very similar to that found here during the first 5 days of the experiment. Thus, even under conditions when reinforcement contingencies do not encourage choosing all arms, substantial alternation still occurs. There are several conclusions to be drawn from these data: First, the rats bring to the experimental situation a strong tendency to perform the task correctly. This is presumably a reflection of spontaneous alternation. Second, the reinforcement contingencies produce an improvement in the number of correct choices, and animals learn to perform better after training. Third, the presence of a spontaneous alternation tendency influences only the baseline level of performance exhibited by the rats and not the strategy used to solve the task; during spontaneous alternation, rats do not choose adjacent arms, chain responses, or utilize intramaze cues (Olton, Note 1).

The sequential position of errors analysis indicated that the tendency to make an error by repeating a response to a particular arm increased with the number of responses made after that arm had been chosen, even when corrections were made for arm preference and for the number of opportunities to repeat each choice. Some of this tendency may have been due to the physical constraints of the apparatus because animals returning to the center platform after choosing an arm were directed away from the arm just chosen. Any such effect would be very minor however. First, the rats often did not go directly to another arm after returning to the center platform; rather, they explored the entrance to several arms, often completing one or more revolutions around the center platform before making a choice. Second, even if the rats did tend to respond to arms physically opposite the one just chosen, following the subsequent choice they would be facing back toward the arm they had just previously chosen. Third, an analysis of response sequences indicated that although there was a slight preference for arms at 90° from the chosen arm, such preferences were weak and not a prerequisite for accurate performance. Thus the tendency for errors to be made by repeating choices early in the day's test was robust and reliable and does not appear to be due to the physical characteristics of the apparatus.

Taken together, these data suggest several conclusions about behavior in the present paradigm. First, the animals had no difficulty identifying the eight spatial locations and remembering whether they had chosen each one; although performance was not perfect, even on the eighth choice it was considerably above chance. Second, there was a small but consistent decrement in performance as the number of choices increased; this decrement was due to interference from previous choices and not just the passage of time. Third, analysis of the effects of prior choices on the probability of an error indicated that the tendency to make an error by repeating a choice was a function of the number of choices since the original correct choice. Fourth, analysis of the effects of prior choices on the location of errors indicated that the eight arms were treated as a separate place and there was no apparent tendency for spatial generalization among arms. Fifth, information was stored choice by choice and repetitions of a choice (i.e., errors) were stored in the same manner as a new choice. Sixth, rats were able to store information from sequential tests separately so that there was no performance decrement of one test on following tests.

Although the present paradigm is operationally different from the usual human paradigms in which primacy, recency, proactive interference, and retroactive interference have been studied, it is nonetheless conceptually very similar. Thus the primacy–recency analysis of human data is similar to the sequential location of errors analysis presented in Table 2. The equivalent of the primacy effect in human serial learning would appear in the present analysis as a lowered probability of an error to choices made early in the test, while the equivalent of a recency effect would appear as a lowered probability of an error to choices made near the end of the test. No indication of a primacy effect was found in any of the analyses. Data from human subjects suggests that the primacy effect may result from more rehearsal of items at the beginning of the list than in the middle; accuracy of recall is a function of the amount of rehearsal, which is greatest for items in the beginning of the list (Rundus, 1971), and when subjects are instructed to rehearse only the most recent item, the primacy effect does not appear (Waugh & Norman, 1965). If the absence of a primacy effect for rodents is a consistent finding, it may suggest that rats do not rehearse their choices or that rehearsal is not preferentially directed toward early choices, in the tasks involved, but equally distributed among all choices. Alternatively, the primacy effect in the human data may be due to the requirement of serial ordering of items and the formation of chains of responses (Mandler & Anderson, 1971). In the present paradigm, of course, rats were not required to chain responses and no particular sequences of choices were preferred over others. Since humans almost inevitably order responses, even when asked to randomize them, a test of this hypothesis can probably be conducted only by testing rats (or some other animal that does not automatically sequence responses), forcing them to choose in a particular serial order and then determining if a primacy effect appears.

The equivalent of proactive and/or retroactive interference in human serial learning would appear in the present analysis as a decrement in performance as the number of choices increased. There was a reliable decrement in performance as the number of choices increased during the day's test, and this decrement appeared even when a correction had been made for the varying chance probability of a correct response. The present experiment is unable to identify definitively whether the performance decrement observed here is due to proactive or retroactive interference because the way in which the animals remember which arms have been chosen and/or which arms have not been chosen has not yet been determined Data from discrimination learning experiments strongly suggest that rats perform discriminations largely on the basis of nonreinforcement (see discussion in Olton, 1972; Olton, Walker, Gage, & Johnson, Note 2). If such is the case in the present experiment, then rats probably remember not to return to arms which have already been chosen because these would result in nonreinforcement if chosen again. The performance decrement would then be due to retroactive interference. Other evidence also supports the suggestion of retroactive rather than proactive interference. The finding of a recency effect in the absence of a primacy effect suggests that the animals are remembering where they have been and which arms ought not to be chosen. The fact that animals could successfully overcome the expected performance decrement when given four or eight tests on each day is also compatible with the suggestion of retroactive interference.

In summary, the results of the present experiments are important because they demonstrate that rats have a remarkable memory capacity for information about spatial location and because they provide information about some of the characteristics of the storage process used in this memory. The data also suggest that just as the introduction of the "interoceptive" learning paradigm (Garcia, Hankins, & Rusiniak, 1974; Rozin & Kalat, 1971) substantially changed our estimate of rats' learning abilities, so the introduction of a spatial location paradigm may change our estimate of rats' cognitive capabilities.

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APPENDIX

TABLE A1

DAILY SEQUENCE OF FIRST EIGHT CHOICES OF RAT 3 IN EXPERIMENT 1

Day	Order	Day	Order	Day	Order
10	85726354	16	76812453	22	76538124
11	85732615	17	83521747	23	86574123
12	58317246	18	86457138	24	64572831
13	68745723	19	45682375	25	46351872
14	73654128	20	63817425	26	46273518
15	75436812	21	86457371	27	84252371

Note. The numbers in the order columns refer to Arms 1–8 of the radial maze.

(*Table A2 is on the next page.*)

TABLE A2

RAW DATA FROM DAYS 1–10 IN EXPERIMENT 4

Rat	Day	Order	Rat	Day	Order	Rat	Day	Order
1	1	76512678365824	5	1	181275317416	9	1	248842137156
	2	71237463845		2	47186325		2	181267478253
	3	75567823451		3	25386417		3	212564718537
	4	67543218		4	16428537		4	73512864
	5	37812456		5	15387264		5	713682514
	6	5678134562		6	1474825136		6	71326485
	7	32187654		7	17534826		7	875136224
	8	35781246		8	14283657		8	15372684
	9	16373135675428		9	73528461		9	28637514
	10	2187573456		10	52831746		10	615247328
2	1	671348625	6	1	75861517356842	10	1	1854231514367
	2	783624715		2	18763518462		2	5724543186
	3	8573246851		3	14678253		3	853658146318572
	4	74568123		4	13457826		4	452641873
	5	687413245		5	67824513		5	35784612
	6	4632571478		6	78135624		6	74318562
	7	67824513		7	78246135		7	17542186
	8	68345712		8	68245713		8	57132468
	9	67814523		9	71352864		9	48154321876
	10	72345186		10	74623815		10	58261437
3	1	7878351246	7	1	76145317528	11	1	2182617354
	2	418514687356142		2	7163816542		2	74318562
	3	712856134		3	4637154218		3	58416273
	4	14536827		4	132518476		4	461742583
	5	42537146217348		5	1358254712356		5	18537462
	6	12437518425417836		6	71435628		6	7685137264
	7	24682467351		7	41725836		7	76845132
	8	24782513476		8	43682571		8	768471523
	9	634671378245		9	58167243		9	8675164432
	10	31753885264		10	57123468		10	68471565723
4	1	18431748751826	8	1	85326487	12	1	1825175346
	2	32164871864325		2	2874173685		2	187428651413
	3	45231786		3	86174318652		3	18178235864
	4	86431752		4	851748623		4	1541265783
	5	48632175		5	86231547		5	613852147
	6	32876541		6	81642753		6	816432567
	7	16572843		7	68725314		7	7625813725814
	8	53286417		8	86571423		8	5686413712
	9	48762513		9	86372541		9	53461248217
	10	65482173		10	86125473		10	31785426

Note. The numbers in the order columns refer to Arms 1–8 of the radial maze.

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Simultaneous and Backward Fear Conditioning as a Function of Number of CS–UCS Pairings

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Four experiments examined the possibility that the outcome of simultaneous and backward fear conditioning procedures might depend upon the number of CS–UCS pairings. A punishment procedure with rats as subjects and shock as the UCS was used; the amount of suppression produced by response-contingent CS presentations indexed the strength of acquired fear. Experiments 1, 3, and 4 examined the suppressive tendencies of simultaneous- and backward-trained CSs after 0, 10, 20, 40, 80, and 160 pairings. The pattern of data suggested that initial pairings have the effect of increasing suppressive tendencies of the CS, while subsequent pairings decrease them. In addition, evidence of fear inhibition was found after 160 pairings in the case of the backward paradigm. Experiment 2 examined several nonassociative accounts based upon differential shock exposure. Groups given 10 pairings or 80 pairings were compared to groups given 10 pairings plus 70 shock-alone presentations. The results indicated that number of pairings, rather than number of UCS occurrences, is the important factor in decreasing the initial suppression. The evidence for eventual inhibition in the backward paradigm suggests that this occurs through the acquisition of inhibitory tendencies which are antagonistic to the previously conditioned excitation.

Historically, investigators of conditioning have been especially interested in the temporal relationship between the conditioned stimulus (CS) and unconditioned stimulus (UCS). But, despite a considerable literature and three extensive reviews in the last two decades (Cautela, 1965; Kimble, 1961; Razran, 1956) there exists little consensus concerning the outcome of conditioning procedures in which the CS does not precede the UCS in time.

Recently, however, there has been considerable interest in earlier proposals by Spooner and Kellogg (1947) and Konorski (1948) that such simultaneous and backward conditioning paradigms result in "inhibitory" associative tendencies. Moscovitch (1972) and Moscovitch and LoLordo (1968) found that CSs trained under these paradigms with aversive UCSs acquired the capacity to suppress fear-motivated instrumental behaviors. Siegal and Domjan (1971) found that simultaneous and backward conditioning procedures later retarded the acquisition of conditioned responding to the CS when it was trained in a forward manner. Also supportive of the inhibitory view are several demonstrations that simultaneously trained CSs attenuate the organism's reactions to the UCS (Segundo, Galeano, Sommer-Smith, & Roig, 1961; Zbrozyna, 1957, 1958).

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Thus, there exists empirical and theoretical justification for the view that simultaneous and backward conditioning procedures result in inhibitory associative tendencies. Furthermore, many of the counterinstances in the reviews of Kimble (1961) and Cautela (1965) might be explained either by the lack of appropriate controls for nonassociative effects (see Rescorla, 1967) or by the insensitivity of the conditioning preparation to inhibitory outcomes.

Directly contrary to the inhibitory viewpoint, however, are the results of several investigations which have succeeded in demonstrating "excitatory" properties to CSs trained under these paradigms. A recent study by Heth and Rescorla (1973), for example, obtained evidence of fear conditioning to simultaneous and backward CSs as compared to treatments involving unpaired CSs and UCSs. Evidence of excitatory tendencies has also been reported by Champion and Jones (1961), Dostalek (1970), Mahoney and Ayres (Note 1), Matsumiya (1960), Mowrer and Aiken (1954), Perlmuter, Kimble, and Leonard (1968), Wagner and Terry (1975), and Zeiner and Grings (1968).

An observation by Pavlov suggests one way in which the existence of excitatory simultaneous and backward conditioning could be reconciled with the inhibitory viewpoint. In discussing the possibility of backward conditioning, Pavlov (1927, p. 393; 1928, p. 381) suggested that any excitatory tendencies acquired by the CS in this paradigm may be transient, with inhibition being the asymptotic outcome. According to this proposition, the demonstration by Heth and Rescorla of excitatory simultaneous and backward conditioning may have reflected preasymptotic tendencies which would have been reduced by additional training.

Some evidence in support of this assertion can be noted (see Champion, 1962; Cohen, 1950; Spooner & Kellogg, 1947). Unfortunately, these studies employed a within-subjects design in which conditioning is assessed through nonreinforced presentations of the CS alone. The possibility of some extinction over the course of training makes the decline in excitatory tendencies which these studies observed rather equivocable. In a somewhat different test of Pavlov's proposal, Nishizawa and Umeoka (1963) examined the conditioned tendencies of a backward CS after 4 or 16 CS-UCS pairings in a between-subjects design; although they did obtain an interaction across levels of training between backward-trained groups and controls, their preparation did not obtain concrete evidence of excitation after 4 pairings, and thus failed to establish the necessary conditions for a test of the hypothesis.

Thus, there exists no conclusive evidence concerning the effect of amount of training on excitatory tendencies in simultaneous and backward conditioning. This investigation explores the issue. Experiment 1 examines the possibility, suggested by Pavlov, that additional training may serve to decrease established excitatory tendencies of a CS conditioned in a simultaneous procedure. Experiments 3 and 4 explore the hypothesis that the amount of training could reconcile some of the excitatory findings of Heth and Rescorla with the inhibitory viewpoint. In addition, several possible explanations of the results are examined. Experiment 2 tests a nonassociative account, while Experiments 3 and 4 are directed towards an associative interpretation.

EXPERIMENT 1

Gantt's translation of Pavlov cited above (Pavlov, 1928) describes the excitatory tendencies of a simultaneous- or backward-trained CS as "evanescent," implying that the excitatory tendencies decreased to a zero level with extended training. Experiment 1 tests a somewhat weaker version of this proposition—that extensive training will reduce excitatory tendencies evident early in training. More formally stated, the hypothesis under consideration is that, given a demonstration of excitatory tendencies in a simultaneous or backward conditioning procedure at some point in training, then at some later point these tendencies will be less.

Experiment 1 tests the above hypothesis using a preparation which assessed the conditioned punishing properties of a CS paired with an electric shock. The extent to which this CS can serve to punish an appetitively motivated instrumental behavior may be considered a measure of its excitatory properties (see McAllister & McAllister, 1971). Heth and Rescorla (1973) studied several temporal paradigms using this preparation, and observed apparent conditioned excitatory tendencies to each after a moderate amount of training. Experiment 1 examines how the excitatory tendencies produced by one of these paradigms are affected by a greater amount of training.

Method

Subjects

The subjects were 64 male Sprague-Dawley rats approximately 100–110 days old at the start of the experiment. They were maintained during the course of the experiment at 80% of their free-feeding weights.

Apparatus

The subjects were run in eight experimental chambers, each contained within a box constructed of sound-attenuating material and equipped with a ventilation fan. During bar-press training and subsequent test sessions the experimental chambers were 22.9 × 20.3 × 20.3-cm operant conditioning boxes. The front and rear walls were made of aluminum; the side walls and top were of Plexiglas. The floors consisted of stainless-steel rods, 1.9 mm in diameter and spaced 19.0 mm apart. The front wall of each contained a recessed food trough located in the center, a stainless-steel lever to the left of it, and a jeweled lamp mounted above the food trough. Pavlovian conditioning training with shock was administered in chambers identical to the operant conditoning boxes in dimensions and design, with the exception that the front wall did not contain a lever, lamp, or food trough.

Shocks were administered to the floor rods by means of a high-voltage, high-resistance shock source delivered through a relay-sequence scrambler of the type described by Hoffman and Fleshler (1962). A light and an 1800-Hz tone could be presented through a 6.5-W bulb and a loudspeaker, respectively, mounted on the inside wall of the sound-attenuation chamber. Programming and recording equipment were located in an adjacent room.

Procedure

Experiment 1 consisted of two replications of 32 animals each. The replications were run 2 months apart, and were treated identically.

Magazine and bar-press training. On the 1st day of the experiment, all subjects were placed in the operant conditioning boxes and given 20 .045-g Noyes pellets on a variable delivery schedule which averaged 1 pellet per minute. In addition, each bar press of the response lever delivered a pellet. The subjects remained in the apparatus until they had made 50 responses and were hand-shaped if necessary. On the next 3 days, all subjects were placed in the operant conditioning boxes for 1 hr and allowed to bar press for food pellets on a 1-min variable interval (VI-1) schedule of reinforcement.

Conditioning. After the 3 days of VI-1 bar-press training, subjects were divided into four groups of eight animals each. Animals of a given group were run at the same time, with each animal assigned to one of the eight operant conditioning boxes. The four groups received 1 more day of bar-press training followed by either 10, 20, 40, or 80 pairings of a 4-sec .5-mA shock UCS and a 2-sec CS consisting of the simultaneous presentation of the tone and light stimuli. The onset of the compound CS occurred 2 sec after the onset of the UCS, and terminated with it. Ten pairings were given during a session, with the intertrial intervals determined by a rectangular distribution of 4, 5, 6, 7, and 8 min; the subjects were taken out 6 min after the last trial. Group 10 received one such conditioning session, Group 20 received two, and so on. The conditioning sessions were staggered, such that all groups completed their conditioning experience on the same day. This was accomplished by interpolating delay periods of different lengths between the 3rd and 4th day of VI-1 training, during which the animals of a given group remained in their home cages and were not run. Group 10 received a delay of 7 days, Group 20 one of 6 days, Group 40 one of 4 days, and Group 80 no delay.

Recovery and testing. Following the conditioning phase, subjects were returned to the operant conditioning boxes and given another 1-hr session of bar pressing on the VI-1 schedule. The next day consisted of a 50 min test session designed to assess the degree to which the CS could serve as a punisher. For the first 30 min of the session, the subjects were allowed to bar press for food on the VI-1 schedule as before. After 30 min had elapsed, punishment was initiated. Each bar press produced a 2-sec CS; a bar press during the CS had no effect upon its duration. The VI-1 food schedule was discontinued during the first 4 min of the punishment phase, but resumed thereafter.

Data analysis. Performance during the test session was assessed by means of suppression ratios (Annau & Kamin, 1961) in the form of $A/A+B$, where A refers to the response rate (in responses/min) during punishment and B refers to the response rate prior to punishment. Thus, a ratio of .50 indicates no suppression, and one of .00 complete suppression. Church (1969) has suggested this measure as a particularly sensitive index of punishment effects. The total number of responses during the 1st half hour was used to determine the prerate. The punishment phase was divided into three periods of 10 min, and separate ratios were computed for each. For the first such period, one response was subtracted from the period total, such that the suppression ratios reflect only the amount of responding after the subject had experienced the punishment contingency.

Since each group of subjects was run in the same set of eight operant conditioning boxes, dependent-group procedures were employed in analyzing the data, with subjects receiving their treatments in the same box considered matched.

Results and Discussion

Data from the two replications are combined for all of the analyses. The mean number of bar presses during the prepunishment period of the test session was 406, 342, 338, and 376, for Groups 10, 20, 40, and 80, respectively. A Friedman one-way analysis of variance revealed no significant differences among the groups in number of bar presses during this period, $\chi^2(3) = .83$, $p > .05$. Consequently, the suppression observed during the 2nd half of the test session can be evaluated against base-line response rates which were comparable for all groups.

Mean suppression ratios for the two 10-min periods of the test phase are plotted in Figure 1. As can be seen in Figure 1, all groups showed some amount of suppression during the punishment phase. This suppression decreased over the course of testing, presumably the result of extinction due to nonreinforced CS presentations.

Since extinction acted to increase variability and to obscure group differences, statistical analyses are reported only for the first 10-min period of the testing phase. The inverse relationship between response suppression and number of pairings is clearly evident in Figure 1. This trend was confirmed by means of a Friedman test over all four groups, $\chi^2(3) = 10.88$, $p < .05$. Individual groups were compared by means of separate Wilcoxon matched-pairs tests. The tests established that Group 10 differed from Group 20 and Group 80, $Ts = 26$ and 10, respectively, $ps < .05$, one-tailed; and that Group 40 differed from Group 80, $T = 23$, $p < .05$, one-tailed. No other comparisons were significant.

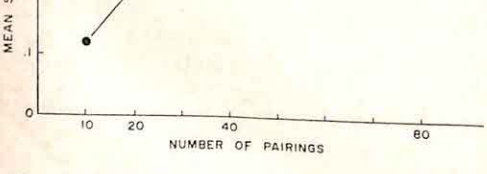

FIGURE 1. Mean suppression ratios for the four groups of Experiment 1. (The groups are arranged along the abscissa according to number of pairings; the parameters of each curve, and their associated symbols, indicate the period of testing from which the points along that curve were calculated.)

These data indicate that the punishment properties of the CS decrease with number of CS–UCS pairings. The function depicted in Figure 1 and the prior report of Heth and Rescorla (1973) suggest the conclusion that the outcome of a given CS–UCS pairing changes from one which *increases* the excitatory tendencies of the CS early in training to one which *decreases* them later in training.

An alternative explanation, however, is possible and it would seem advisable to consider it before pursuing the first in any greater detail. The attenuation observed in Experiment 1 may not be due to the additional pairings per se, but rather to factors related solely to the presentation of the individual events. Experiment 2 examined the sufficiency of UCS-alone presentation.

EXPERIMENT 2

There are several reasons why UCS presentations alone might have had the effect of attenuating the punishing properties of the CS in Experiment 1. For one, since the shocks in the paradigm of Experiment 1 are unsignalled, contextual cues may acquire conditioned tendencies. If these cues are incompletely extinguished prior to testing, then the CS will be assessed against a context which is itself excitatory. Since groups different in number of CS–UCS pairings in Experiment 1 also differed in number of shocks, the CS might have been assessed against contexts of different excitatory strengths.

Another explanation appeals to habituation produced by shock presentations. Rescorla (1973) found habituation of the UCS to attenuate the CR in a conditioned suppression situation. Since the habituation operation in Rescorla's study consisted of repeated UCS presentations, the additional pairings given Groups 40 and 80 may have functioned to habituate the UCS, and thus attenuate the previously established CR.

While stressing different mechanisms, both of these accounts suggest that, had Groups 10 and 80 been equated for number of shocks, their CSs would have demonstrated equivalent punishing properties. If, on the other hand, additional CS–UCS pairings are necessary for the attenuation effect, then these two groups should appear different despite having been equated on number of shocks. Accordingly, Experiment 2 examines the effect of shock-alone presentations upon the punishing properties of the simultaneous CS of Experiment 2.

Method

Subjects and Apparatus

The subjects were 35 male Sprague-Dawley rats of the same age and maintained in the same manner as those of Experiment 1. The apparatus and stimuli were the same, although only seven of the eight boxes were used.

Procedure

Subjects were given 1 day of magazine training, and 4 days of VI-1 training as in Experiment 1. Following this, five groups of seven subjects each were formed. Group 80 received 80 pairings of the 4-sec shock and the 2-sec tone–light compound used in Experiment 1. The interval between the onset of the shock and the onset of the CS (UCS–CS interval) was 2 sec; the distribution of intertrial intervals and the length of the session were likewise identical to Experiment 1. Group 10/70 received 10 such pairings on the day after pretraining, followed by 7 days on which shock alone was presented according to the same schedule as Group 80. Group 70/10 received the reverse of this treatment, with the 70 shock-alone presentations preceding the 10 shock-plus-CS pairings. Two groups were given 10 pairings only. Group 10/0 received these pairings at the same time as Group 10/70 and then spent the next 7 days in their home cages, while Group 0/10 spent the first 7 days after pretraining in their home cages and received their 10 pairings on the 8th day along with Group 70/10. All groups received a 1-hr recovery session on the day following the conditioning phase, during which they bar pressed for food on the VI-1 schedule. On the following day the punishing properties of the tone–light CS were tested during a 1-hr session in the same manner as Experiment 1. The punishment contingency was in effect for 30 min.

Results and Discussion

The mean number of responses during the first half of the test session was 396, 329, 357, 368, and 322 for Groups 10/0, 10/70, 70/10, 0/10, and 80, respectively. One subject of Group 0/10 failed to recover the bar-pressing response following the conditioning treatment. A subject from Group 10/0 inexplicably stopped responding during the prepunishment phase of testing. Data from these two animals were excluded; statistical comparisons involving these two groups were adjusted by likewise excluding the corresponding subjects of the other groups for those comparisons.

The groups did not differ significantly in number of responses during this period, $\chi^2(4) = 2.40$, $p > .05$. The mean suppression ratios for each group over the course of testing are given in Table 1. The general trend was that Group 80 appeared less suppressed than the other four groups, which did not appear to differ among themselves.

Statistical comparisons between groups during the first 10-min period confirmed this impression. Group 10/0 and 0/10 were significantly more suppressed than Group 80, $Ts = 0$, $ps < .05$, one-tailed, which replicates the basic finding of Experiment 1. Furthermore, these two groups did not differ significantly, $T = 4$, $p > .05$. Thus, the attenuation effect can be observed in the absence of any difference between the last day of VI training and the first day of Pavlovian conditioning.

The above comparisons indicate that the 70 additional pairings which Group 80 re-

TABLE 1

MEAN SUPPRESSION RATIOS FOR THE TESTING PERIODS, EXPERIMENT 2

Group	Period 1	Period 2	Period 3
10/0	.313	.424	.476
10/70	.254	.454	.514
0/10	.276	.464	.516
70/10	.331	.442	.490
80	.450	.525	.542

ceived relative to Groups 0/10 and 10/0 clearly reduced the initial excitatory tendencies. The question of interest from the nonassociative interpretation of this effect is whether 70 shock-alone presentations would produce an equivalent effect. It is quite clear from Table 1 that they did not. To evaluate the effect of additional shock-alone presentations, Group 10/0 may be compared to Group 10/70, and Group 0/10 may be compared with Group 70/10. Neither of these comparisons was significant, $Ts = 10$ and 7, respectively, $ps > .05$, one-tailed. Indeed, there is some indication from the means of Table 1 that shocks subsequent to the pairings increased suppression. It can be noted that this is in contrast to the habituation effect reported by Rescorla (1973) in a CER situation.

Examining those groups which received equivalent numbers of shocks but different numbers of pairings also reveals evidence against a purely nonassociative interpretation based upon differential shock experience. Both Groups 70/10 and 10/70 were more suppressed than Group 80, $Ts = 0$ and 1, respectively, $ps < .05$, one-tailed. The fact that they differed from Group 80 indicates that the attenuation effect is demonstrable when animals are matched on number of shock presentations. If shock presentations alone were responsible for the effect, then these groups would have shown equivalent amounts of suppression.

Clearly, Experiment 2 demonstrates that differential shock experience alone is not responsible for the attenuation effect. Thus it would appear more reasonable to interpret the attenuation effect of Experiment 1 in terms of different consequences of CS–UCS pairings at different points in training if it is assumed that a CS given no CS–UCS pairings would have demonstrated less excitatory tendencies than one given 10. While this appears reasonable from the data of Heth and Rescorla (1973), the nonmonotonic character of the acquisition function could be more conclusively established by examining the excitatory tendencies of the CS at the start of training. Experiments 3 and 4 of this investigation examined the nature of the simultaneous and backward acquisition function in more detail.

EXPERIMENT 3

Pavlov's commentary on simultaneous and backward conditioning not only described the excitatory tendencies as "evanescent" but also suggested that the eventual outcome of these procedures was a stimulus which was inhibitory. Although Pavlov's usage of the term "inhibition" is not completely clear, it does suggest that CS–UCS pairings late in training may serve to decrease the punishing properties of the CS through the acquisition of conditioned tendencies antagonistic to any excitatory tendencies. Indeed, these pairings may be sufficient to endow the CS with net inhibitory associative strength, as Pavlov suggested (see Nishizawa & Umeoka, 1963, for a similar proposal; also, cf. Wagman & Allen, 1964, for the converse of this).

This would suggest a biphasic acquisition function, according to which the CS would initially exhibit conditioned excitatory tendencies, but after extensive training would exhibit conditioned inhibition. Experiment 3 examines this hypothesis by testing for both inhibitory and excitatory conditioned tendencies of a CS after various amounts of training. The following procedure was employed: All subjects were first given 10 pairings of the light stimulus and the shock UCS. Following this training, separate groups received different experiences with respect to the tone. Three groups received 10, 80, and 160 pairings of the tone and shock. Two groups were run which did not receive tone–shock pairings: One received no additional training, while the other received 160 shocks to control for the extensive UCS exposure of the 160 pairings group. The suppressive properties of the tone and light presented in compound were then assessed.

It was felt that this procedure would be sensitive to both excitatory and inhibitory response tendencies to the tone relative to the control procedures. Specifically, if the tone is excitatory, the compound should be more suppressive than that of the nonpaired controls; if it is inhibitory, then the compound should be less suppressive than that of the nonpaired controls (Reberg, 1972). The hypothesis under consideration would predict that subjects given 10 tone–shock pairings would show more suppression to the compound than the group given no further tone training, and that subjects given 160 pairings would show less suppression than groups given no training or shock exposure only. The compound procedure was intended to insure measurable suppression from which inhibition might be detected as a decrease. The response tendencies of the tone alone, however, were also tested.

Method

Subjects and Apparatus

The subjects were 40 male Sprague-Dawley rats of the same age, and maintained in the same manner, as those of Experiment 1. The apparatus was the same.

Procedure

Magazine and bar-press training were administered in the same manner as Experiment 1. VI-1 training was given for 4 days. Following this training, the animals were divided into five groups of eight subjects each. Each group of subjects then received 1 day of light conditioning, consisting of 10 pairings of the 4-sec shock and a 2-sec presentation of the 6.5-W light. The UCS–CS interval was 2 sec as in Experiment 1; the distribution of intertrial intervals and the length of the session were likewise the same. Following the light conditioning phase, subjects received different conditioning experiences with respect to the tone CS. Groups 10, 80, and 160 received 10, 80, or 160 pairings, respectively, of the shock and a 2-sec presentation of the 1800-Hz tone. The UCS–CS interval was 2 sec; 10 pairings were given each day as in Experiment 1. Two additional groups were run: Group 160Sh received 160 shock-alone presentations according to the trial schedule of Group 160; Group 0 received neither shocks nor tones during this phase. The conditioning experience was staggered with the same intent as Experiment 1. This was accomplished by delays interposed between light and tone conditioning, during which the animals of a given group were placed in the conditioning chambers but received no programmed stimuli. Group 0 received 16 days of such treatment, Group 10 received 15 days, and Group 80 received 8. Following the conditioning phase, two bar-press recovery sessions on the VI-1 schedule were given. These were followed by two test sessions—the first intended as an assessment of the punishing properties of the light and the tone presented simultaneously, and the second to assess the tone alone. Consequently, the punisher on the first test session was a 2-sec tone–light compound, while on the second it was a 2-sec presentation of the tone alone. The testing procedure was otherwise identical to Experiment 2.

Results and Discussion

The two test sessions will be dealt with separately. The first evaluated the punishing properties of the tone–light compound. The mean number of responses during the prepunishment period of this test was 475, 506, 460, 521, and 409, for Groups 0, 10, 80, 160, and 160Sh, respectively. A Friedman analysis of these prerates was nonsignificant, $\chi^2(4) = 2.60$, $p > .05$. The mean suppression ratios of each group are plotted in Figure 2 for the three testing periods.

The tone–light test examined the proposition that the tone acquired inhibitory tendencies with extended training. Evidence of inhibition would consist of less suppression to the tone–light compound in a paired group as compared to the appropriate controls. Group 160 can be compared with two groups which did not receive tone–shock pairings (Groups 0 and 160Sh). During the first 10-min punishment period, Group 160 was somewhat *more* suppressed than Groups 0 and 160Sh. However, neither comparison was significant, $Ts = 10$ and 14, respectively, $ps > .05$, one-tailed. Group 80 can be compared to Group 0. This comparison was not significant, $T = 14$, $p > .05$, one-tailed, but was likewise contrary to an inhibitory prediction.

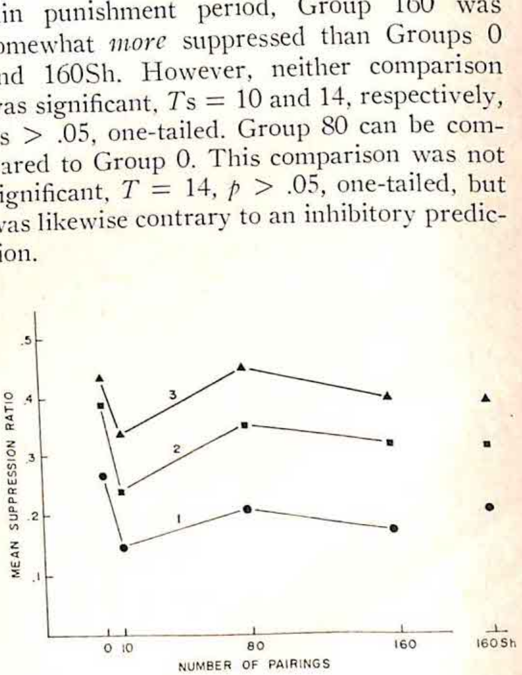

FIGURE 2. Mean suppression ratios for the five groups of Experiment 3 on the tone-light compound test. (The groups are arranged along the abscissa according to the number of pairings.)

The question of initial excitatory tendencies can be addressed by examining the data of Group 10. These animals were somewhat more suppressed than the other four groups. During the first period, this group differed significantly from Group 0, $T = 0$, $p < .05$, one-tailed, but not from Group 160Sh, $T = 15$, nor from the other two paired groups, $T\text{s} = 14$ and 13.5 for the comparisons with Groups 80 and 160, respectively. Since all groups received equal amounts of training to the light alone, the greater amount of suppression observed in Group 10 as compared to Group 0 is consistent with an interpretation in terms of differences in the amount of excitation to the tone. However, there was, as in Experiment 2, indication that suppression was increased by additional shocks (comparing Group 0 and Group 160Sh). This clouds the interpretation of the difference between Group 10 and Group 0, but may also explain why the tendency for Groups 80 and 160 to suppress less than Group 10, as seen in Experiments 1 and 2, was not reliable in this instance.

The second test session examined the conditioning to the tone by presenting the tone alone as the conditioned punisher. The mean number of responses during the prepunishment period was 516, 497, 466, 525, and 410, for Groups 0, 10, 80, 160, and 160Sh respectively. The mean suppression ratios of these groups are plotted in Figure 3 for each 10-min period of the test session.

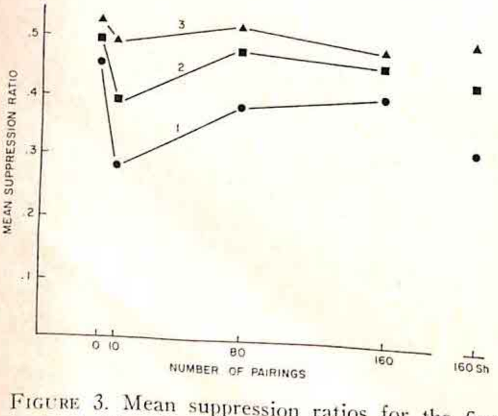

FIGURE 3. Mean suppression ratios for the five groups of Experiment 3 on the tone-alone test. (The groups are arranged along the abscissa according to number of pairings.)

Since the data of primary interest concern the effect of increasing the number of CS–UCS pairings, the overall analyses were conducted on Groups 0, 10, 80, and 160. These groups did not differ significantly in their prerates, $\chi^2(3) = 2.25$, $p > .05$. Extinction was quite rapid, and group differences were most pronounced during the first 10-min period. Figure 3 shows a nonmonotonic trend in the amount of suppression over increasing numbers of CS–UCS pairings. This trend was confirmed by statistical analysis of the first 10-min period. A Friedman test on these four groups was significant, $\chi^2(3) = 10.05$, $p < .05$. During the first period, Group 10 was significantly more suppressed than Groups 0 and 160, $T\text{s} = 0$ and 5, $p\text{s} < .05$, one-tailed. Group 10 was also more suppressed than Group 80, but this comparison was not significant, $T = 8$, $p > .05$, one-tailed. No other differences were significant. Not included in the overall analysis are the data for Group 160Sh; however, first period comparisons of this group with all other groups were not significant (smallest T, for the comparison with Group 0, was 7).

Thus, Experiment 3 more extensively maps the acquisition function of the simultaneous paradigm studied in Experiment 1. That function apparently is characterized by initial increases in suppression followed by decreases. The tone–light compound test also indicates that this paradigm does not result in net inhibitory strength after 160 pairings.

Nevertheless, Experiment 3 does not rule out the possibility of eventual inhibition in other simultaneous and backward paradigms. In a study of several such paradigms, Heth and Rescorla (1973) observed less excitatory tendencies to paradigms with longer intervals between the onset of the UCS and the onset of the CS. This might reflect either a decreasing effectiveness of the excitatory component of simultaneous and backward conditioning or, perhaps, a corresponding increase in the inhibitory component. In this connection, it can be noted that Moscovitch and LoLordo (1968) observed greater inhibitory tendencies to a CS trained in a backward paradigm than one trained in a cessation relationship. This suggested that Experiment 3 should be replicated using a backward conditioning paradigm. It may be the case that, unlike the simultaneous paradigm of Experiment 1, other paradigms may show eventual inhibition.

EXPERIMENT 4

Experiment 4 examined the possibility of eventual inhibition in one of the backward paradigms of Heth and Rescorla (1973). The paradigm chosen was one in which the onset of the CS occurs .5 sec after the termination of the UCS. Heth and Rescorla found this paradigm to apparently endow the CS with excitatory properties after 20 pairings. The question of interest to Experiment 4 was whether the CS would be inhibitory after 160 pairings. Consequently, the 160 and 160Sh groups of Experiment 3 were replicated using the backward paradigm. To provide some indication of the acquisition function of this paradigm, two additional points in training were assessed by replicating Groups 0 and 10 of Experiment 3 as well.

Method

Subjects and Apparatus

The subjects were 32 male Sprague-Dawley rats of the same age and maintained in the same manner as those of Experiment. 1. The apparatus and stimuli were the same.

Procedure

The procedure was similar to that of Experiment 3, the major exception being the type of training given to the tone. The subjects were given 1 day of magazine training, followed by 4 days of VI-1 training. They were then divided into four groups of eight. Each group received 10 pairings of the light and shock on the day following the last day of VI-1 training. The light conditioning phase was identical to that of Experiment 3, with the 2-sec light presented 2 sec after the onset of the 4-sec shock. Following the light conditioning phase, subjects received different conditioning experiences with respect to the tone CS. Groups 10 and 160 received 10 or 160 backward pairings, respectively, of the 4-sec shock and a 2-sec presentation of the 1800-Hz tone. The onset of the tone occurred .5 sec after the termination of the shock. Ten pairings were given each day in the same manner as Experiment 1. Two additional groups were run: Group 160Sh was given 160 presentations of the shock alone according to the same schedule as Group 160; Group 0 received neither shocks nor tones during this phase. As in Experiment 3, the groups were staggered through delay sessions interposed after the light conditioning phase, during which the animals were placed in the conditioning chambers but received no programmed stimuli. Group 0 received 16 such sessions, while Group 10 received 15. Following the tone conditioning phase, subjects were given two barpress recovery sessions on the VI-1 schedule. Following this, the punishing properties of the tone–light compound were assessed using the simultaneous presentation of the 2-sec tone and the 2-sec light. A test of the tone alone was not given.

Results and Discussion

Mean number of responses during the prepunishment period was 542, 491, 504, and 483 for Groups 0, 10, 160, and 160Sh, respectively. A Friedman test of the prerates was not significant, $\chi^2(3) = .75$, $p > .05$. The mean suppression ratios of each group are plotted in Figure 4 for the three testing periods. As in the preceding experiments, the analysis reported will be confined to the first 10-min period.

The nonmonotonic trend observed in Experiment 3 is more apparent in Figure 4. A Friedman test of all four groups was significant, $\chi^2(3) = 9.15$, $p < .05$. Furthermore, Group 10 was significantly more suppressed than the other three groups, $Ts = 0$, 3, and 5 for the comparisons with Groups 0, 160, and 160Sh, respectively, $ps < .05$, one-tailed. Thus, the data of Experiment 4 suggest an acquisition function similar to that of the preceding experiment.

As in Experiment 3, the question of inhibition can be addressed by comparing the punishing properties of the tone–light compound of Group 160 with those exhibited by the

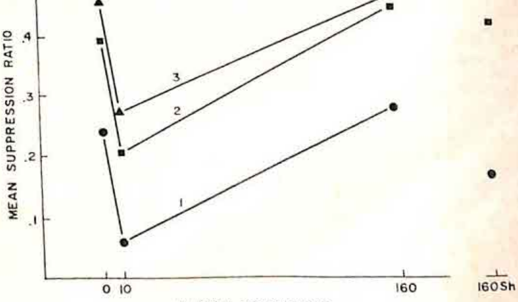

FIGURE 4. Mean suppression ratios for the four groups of Experiment 4. (The groups are arranged along the abscissa according to number of pairings.)

groups which received no explicit training on the tone. Group 160 was less suppressed to the compound than was Group 160Sh. This comparison was significant, $T = 4$, $p < .05$, one-tailed. Group 160 was also slightly less suppressed than Group 0, but this comparison was not significant, $T = 15$, $p > .05$, one-tailed. As noted in Experiment 3, the additional shock which Group 160 received may have somewhat affected the latter comparison, making the 160 versus 160Sh comparison a more appropriate test of inhibition.

It is of interest to compare the suppression observed to individual groups of Experiment 4 with those of Experiment 3. Examining the amount of suppression during the first 10-min period of the tone–light compound test, the two 0 groups showed highly similar suppression ratios (.27 and .24 for Experiments 3 and 4, respectively). Animals in Group 10 of Experiment 4 were more suppressed than their counterparts in Experiment 3, with a ratio of .06 compared to one of .15. This difference was not significant, $T = 11$, $p > .05$. After 160 pairings, this pattern was reversed, with Group 160 of Experiment 4 showing less suppression (.28) than Group 160 of Experiment 3 (.17). However, this difference was not significant either, $T = 10$, $p > .05$, one-tailed. Nevertheless, there does appear some indication that the decremental effects of the 160 pairings was greater in the backward procedure of Experiment 4 than in the simultaneous procedure of Experiment 3. To evaluate this possibility, the differences between the suppression observed to a subject of Group 10 and its counterpart in Group 160 were computed for both Experiments. A Mann-Whitney test of these scores showed that the differences of Experiment 4 were significantly larger than those of Experiment 3, $U = 14$, $p < .05$, one-tailed. It can also be noted that evidence of inhibition was observed in the former case, but not in the latter. The observation that the magnitude, and possibly the asymptote, of this extended training effect seems to depend upon the temporal relation which the tone bears to the shock is consistent with an associative interpretation of the attenuation.

The data of Experiment 4, therefore, support the hypothesis of a biphasic acquisition function, with the CS controlling excitatory tendencies early in training and inhibitory tendencies later. Before this proposal concerning the associative nature of these response tendencies is unequivocably accepted, however, it is necessary to examine whether all possible nonassociative accounts of the data have been excluded by the control procedures used in Experiments 3 and 4.

It could be argued, for example, that Group 0 fails to completely control for nonassociative suppressive factors in Group 10, since the latter group received tone–shock training 3 days prior to the first test session while the former did not. The differences in number of shocks and time since the last shock might be a source of differential pseudoconditioning. According to this view, a more appropriate control against which to assess excitatory tendencies would be one which receives shock alone at the same time as Group 10.

A comparison along these lines is possible, however. Like Group 10, Group 160Sh received shock exposure 3 days prior to testing. Group 10 was significantly more suppressed than Group 160Sh in Experiment 4, despite the fact that differential recency of shock could not be a factor in this comparison. Furthermore, Group 0 and Group 160Sh together provide two extreme points of UCS exposure on either side of Group 10. That Group 10 is more suppressed than either strongly supports the inference of excitatory associative tendencies in the paired group.

Experiment 4 also provided evidence of inhibition after extensive training in the comparison of Group 160 with Group 160Sh. These groups were equated in number of shocks—eliminating one potential nonassociative source of differential suppression. Other accounts are possible, however. For example, unconditioned suppressive properties of the CS could be especially affected by its novelty; extensive pairings may "habituate" or "familiarize" the tone and hence reduce these nonassociative contributions to the suppressive power of the tone–light compound. It can be noted that familiarization alone would not appear to be sufficient to account for the decline in suppression over conditioning trials, since Experiment 3 successfully demonstrated a difference between Groups 10 and 160 on the tone–alone test after both groups had received extensive experience with the tone on the previous day's test of the tone–light compound. Unfortunately, an explicit control for familiarization of the CS was not included in Experiment 4. Thus, while a reduction in novelty appears insufficient by itself to account for the attenuation in suppression observed over training, the extent to which it contributes to the 160 versus 160Sh comparison of Experiment 4 remains to be determined. Until this nonassociative account is more conclusively eliminated, the identification of inhibition in Group 160 must remain tentative.

GENERAL DISCUSSION

This investigation found simultaneous and backward conditioning procedures to result in the nonmonotonic acquisition of punishing properties by the CS over trials. It was suggested that this acquisition function reflected an underlying change in the associative tendencies controlled by the CS. This interpretation was tested in Experiments 3 and 4 of this investigation, which found evidence indicating that the initial increase in punishing properties represents the acquisition of excitatory associative strength, and the subsequent decrease in suppression to be a decrement in that excitation—possibly through the growth of conditioned inhibition.

Such a conclusion would suggest several important considerations in evaluating the outcome of simultaneous and backward conditioning procedures. As noted above, excitatory tendencies resulting from these procedures might only be demonstrable at certain presasymptotic levels of conditioning. It is perhaps noteworthy that, with the exception of Dostalek (1970), the most successful demonstrations of excitation (Champion & Jones, 1961; Heth and Rescorla, 1973; Matsumiya, 1960; Mowrer & Aiken, 1954; Perlmuter, Kimble, & Leonard, 1968; Wagner and Terry, 1975; Zeiner & Grings, 1968; and Mahoney and Ayres, Note 1) have been reported in experiments using 24 CS–UCS pairings or less. The results of this investigation suggest these findings could be reconciled to the inhibitory viewpoint of simultaneous and backward conditioning through training variables.

Experiments 3 and 4, however, indicate the influence of other factors as well. As discussed above, a comparison of Experiments 3 and 4 suggests the rate of decrement was somewhat different in the two paradigms studied. In addition, no evidence of inhibition was observed after 160 pairings in the case of the simultaneous paradigm of Experiment 3, while inhibition was indicated using the backward paradigm of Experiment 4. This might reflect the greater importance of postUCS termination events in the decremental process, although a more detailed model of the temporal relationships involved would be necessary to such a conclusion. The data do suggest a very dynamic situation in which a whole family of acquisition functions might be generated by variations in the UCS–CS interval. Whether these functions would have the same nonmonotonic characteristics as those depicted in Figures 2 and 4 would have to be determined by a more extensive investigation. For example, Siegel and Domjan (1974), using different paradigms and preparations than those of Experiment 1, failed to find any evidence of preasymptotic excitatory conditioning over a large range of training values. The additional factors involved would have to be identified through a comprehensive series of parametric studies.

The data of Experiments 3 and 4 present some fundamental implications for any understanding of what is learned on a given simultaneous or backward conditioning trial. If an associative explanation is accepted, then the theoretical problem is to specify how the associative tendencies of the CS are affected by individual conditioning trials. As noted, the nonmonotonic acquisition functions of Experiments 3 and 4 suggest the presence of two qualitatively different consequences at different points in training. Clearly, the theoretical challenge of this assertion is to identify what is responsible for these changes in the consequence of pairings over trials.

This issue is especially problematic for those theories which assume an independence of path in the determination of the outcome of a given conditioning trial. Briefly stated, this assumption holds that the outcome of a trial is determined by the associative strength of the CS at the time of the trial, irrespective of the previous history of training which established that level of strength. While this assumption is commonly adopted by most linear conditioning models, it is vitiated by the nonmonotonic acquisition functions observed in this investigation. These indicate that the outcome of a given trial is not uniquely determined by the level of associative strength at the time of that trial, since certain levels of strength occur on either side of the function's inflection point.

This difficulty could be handled in several ways. Wagner and Rescorla (1972) have noted that certain violations in the independence of path assumption can be treated within a conditioning model which assumes that the consequences of a given trial are determined by the total associative strength of the aggregate stimulus complex at the time of the trial. Within this model, different conditioning histories may have different effects upon cues of equal associative strength via the differential contribution of other cues present at the time of a trial. Indeed, under certain conditions, this type of model could generate the sort of nonmonotonic curve found in Experiments 3 and 4; these conditions are satisfied when a stimulus presented in compound with the CS during a pairing is also independently reinforced (see Rescorla, 1972). Further research is necessary to determine whether such additional cues can be identified in the case of simultaneous and backward conditioning and whether their trial-by-trial changes in associative strength fall within the boundary conditions of this explanation.

Another possible solution would be to abandon a unitary associative construct in favor of two independent and opposing associative tendencies. Konorski (1948), for example, has proposed that stimuli which are coincident with an increase in stimulation from the UCS acquire excitation, while stimuli which are coincident with a decrease in stimulation, such as that which is afforded by the termination of the UCS, acquire inhibition. Somewhat similar proposals have been advanced by Mowrer (1960), Denny (1971), and Solomon and Corbit (1974).

Within this framework, it does not seem implausible that both processes are present to some degree in any CS–UCS pairing, with their respective contributions towards response tendencies determined by the temporal relations of the paradigm. The nonmonotonic acquisition functions of simultaneous and backward conditioning could then be generated by different rates of acquisition of the two, with the initial incremental phase the consequence of faster acquisition of excitatory tendencies. This description differs from conventional treatments of conditioning in that it suggests the experimental possibility that a given conditioning trial may result in the concurrent acquisition of excitatory and inhibitory tendencies. Although this is a somewhat unusual proposition, it would not seem to be precluded given complete independence of the two.

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Learned Irrelevance and Learned Helplessness: Rats Learn that Stimuli, Reinforcers, and Responses are Uncorrelated

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In a series of four experiments, preexposure to uncorrelated presentations of noises and shocks retarded the acquisition of conditioned emotional response (CER) and signaled punishment suppression when this preexposure was carried out while the rats were not permitted to respond. When preexposure was carried out while the rats were responding, preexposure to uncorrelated noises and shocks interfered with signaled punishment suppression on the first test day, and both preexposure to uncorrelated noises and shocks and preexposure to the shocks caused similar interference on the remaining days of the test. Signaled punishment suppression was interfered with more when preexposure to uncorrelated noises and shocks was carried out while the rats were responding, but CER suppression was similarly affected whether the rats were permitted to continue responding or not. These results suggest that rats may learn that both noises and shocks and responses and shocks are uncorrelated, and these two types of learning may interfere with learning CER suppression and the punishment contingency, respectively.

Rescorla (1967) and Prokasy (1965) have suggested that the uncorrelated or "truly random" procedure, in which a conditioned stimulus (CS) and unconditioned stimulus (US) are presented with no temporal relation to one another, is a useful control procedure for classical conditioning experiments. Rescorla (1966) described an experiment in which dogs, after being trained to carry out Sidman avoidance in a shuttlebox, were given one of three types of conditioning procedures while restrained in one side of the shuttlebox. The animals in the uncorrelated group received a number of temporally unrelated tone and shock presentations. The animals in the positive correlation group received the same treatment as the uncorrelated group except that all shocks that did not occur in the 30 sec following each tone were omitted. For the negative correlation group only those shocks that occurred in the 30 sec following the tone were omitted. Later, when the dogs were again avoiding shocks and the tone was presented, response rate increased for those animals that had received the positive correlation and decreased for those animals that had received the negative correlation. The result of present interest, however, is that the response rate of those animals that had received the uncorrelated procedure was little affected by the tone. Rescorla (1967) interpreted data such as these to imply that this uncorrelated procedure produced little or no associative learning and yielded a good neutral baseline from which the occurrence of Pavlovian conditioning could be measured. This notion of the associative neutrality of the random procedure was later more clearly specified in Rescorla's (1969) contingency theory of Pavlovian conditioning. Under certain restricted conditions it is also an implication of Rescorla and Wagner's (Rescorla & Wagner, 1972; Wagner & Rescorla, 1972) model of associative learning.

Some experiments by Mackintosh (1973) lead to an alternative view of the effects of the uncorrelated procedure. In a typical

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experiment thirsty rats were placed in an apparatus that contained a drinking tube. First, they were trained to lick the tube to receive water delivered on a variable interval schedule. During the next phase (preexposure) of the experiment, one group received uncorrelated presentations of tones and shocks, a second group was exposed to the tones only, a third, to the shocks only, and the final group received no presentations of either stimulus. Following this phase the animals were trained on a standard conditioned emotional response (CER) procedure. This is a classical conditioning paradigm in which a CS (in this case, the tone) signaling inescapable shocks is presented to a subject responding on an appetitive instrumental schedule. The typical conditioned response to such a procedure is a suppression of the instrumental response during the CS. The major finding of this experiment was that the uncorrelated group acquired CER suppression more slowly than the other groups. Mackintosh (1973) suggested, in a view that was hinted at by Rescorla (1967, p. 77), that the uncorrelated procedure was not associatively neutral but that the animals could learn the specific lack of correlation that it entailed. Such "learned irrelevance" could interfere with future conditioning to that CS and US.

The experiments and theories of Mackintosh and Rescorla deal mainly with classical conditioning. However, a number of loosely analogous experiments dealing with escape–avoidance learning in dogs have been reported by Maier, Seligman, and Solomon (1969; Seligman, Maier, & Solomon, 1971). Typically in these experiments restrained dogs were given a series of severe inescapable shocks before being given the opportunity to escape or avoid shock in a two-way shuttlebox. The main finding was that the dogs that were preexposed to unavoidable shock took longer to acquire escape–avoidance behavior than nonpreexposed controls. Maier et al. (1969) suggested that this interference was the result of "learned helplessness." The dogs preexposed to inescapable shock learned that their behavior did not affect the presentation of the shocks and hence were unlikely to learn that the later shocks, in the shuttlebox, were avoidable.

Demonstration of learned helplessness with rats has been quite difficult. In fact, Weiss, Krieckhaus, and Conte (1968) were led to suggest that learned helplessness might be specific to dogs and that the small effects found in rats were a result of conditioned freezing learned during the preexposure period. Maier, Albin, and Testa (1973) have reported that a strong learned helplessness effect in rats depends on the use of responses, such as double crossing of the shuttlebox or turning a wheel, which are learned more slowly than single crossing in the shuttlebox. Although this demonstration does not rule out the possibility that "learned helplessness" in rats may be the result of conditioned freezing, its results make this explanation less plausible. It is difficult to see why double crossing of the shuttlebox should be interfered with by conditioned freezing and single crossing should not.

Preexposure to shocks and CSs has thus been shown to interfere with the later learning of both Pavlovian conditioned responses and instrumental escape–avoidance responses. It seems plausible to suppose that the two effects are related. The present experiments were designed both to evaluate the reliability of both effects and also, by studying each under comparable conditions, to elucidate the relationship between them.

The procedures used in the present experiments were the CER and signaled punishment. The signaled punishment procedure closely resembles the CER procedure, with the sole exception that shocks are delivered in the presence of the CS or signal only if the subject responds. In CER, therefore, there is a classical contingency between CS and shock; in signaled punishment there is a further instrumental contingency in that the delivery of shock is contingent on the occurrence of a response in the presence of the CS. The particular CER schedule used here was similar to that employed by Church, Wooten, and Mathews (1970), in which shocks were programmed on a variable time schedule throughout each CS rather than occurring only at the termination of the CS. This procedure was chosen to increase the similarity between CER and the signaled punishment procedure in which shocks were programmed on a variable interval (response–contingent) schedule throughout each CS.

Although the main reason for using signaled punishment as the instrumental paradigm in the present experiments was that it is formally similar to a particular classical paradigm, there were other reasons for its use. First, learned helplessness has so far been studied only in instrumental escape or avoidance paradigms. If the effect is of any generality, it should be possible to obtain in other situations. Second, if uncorrelated presentations of CS and US could be shown to interfere with the suppression of responding on a signaled punishment schedule, this would be difficult to reconcile with the conditioned freezing hypothesis advanced by Weiss et al. (1968). Subjects that have learned to freeze in the presence of a CS may fail to learn an active avoidance response but should show more rather than less suppression of responding in a signaled punishment situation.

EXPERIMENT 1

This experiment was an attempt to demonstrate, in similar preparations, that the uncorrelated procedure could interfere with the acquisition of both CER and signaled punishment suppression. After being trained to press a lever for food delivered on a variable interval schedule, two groups of rats were preexposed to uncorrelated presentations of shocks and noises, whereas two other groups received no such preexposure. In the later test phase of the experiment, one preexposed group (Group Noise-Shock/CER) and one nonpreexposed group (Group CER) received a CER procedure, whereas the other two groups (Group Noise-Shock/Punishment and Group Punishment) received a signaled punishment procedure.

Method

Subjects. The subjects were 32 male hooded Long-Evans rats which were shipped to the laboratory, from the Quebec Breeding Farms, at weights varying from 250 g to 300 g. The rats were allowed to adapt to the laboratory conditions with free access to Purina Rat Chow and water for 5 days. They were then weighed and reduced to 80% of their free-feeding weights for the remainder of the experiment.

Apparatus. The four conditioning chambers were 23-cm high, 20-cm deep, and 25-cm wide. One side wall of each was transparent Perspex, the ceilings were translucent Perspex, and the three remaining walls were sheet metal. In each chamber one end wall contained a lever, a food cup, and an acoustic speaker. The lever was a Lehigh Valley retractable lever which was centered 4 cm from the metal side wall and 4 cm above the grid floor. The food cup was located behind a 4-cm^2 hole, which was centered 6 cm from the Perspex wall and 4 cm above the grid floor. A Ralph Gerbrands pellet dispenser could deliver 45-mg Noyes food pellets into the cup. The speaker was centered in the wall 16 cm above the grid floor and could present the auditory CS, which was a 75-db. white noise re 20 μN/m^2 (background 65 db.) from a Grason Stadler Model 901 B noise generator. Scrambled shock could be delivered through the grid floor, lever, and metal walls from a Grason Stadler Model 900 shock generator. Shocks were .25 mA in intensity and .5 sec long. A very dim houselight (40 W 110 vac bulb in series with a 1,000-Ω resistor) glowed above the translucent ceiling of the chamber. The chambers were located in separate sound and light attenuating chests which were located in a separate room from the automatic programming and recording equipment.

Procedure. The rats were trained to press the lever for pellets over 3 days. On the first day the levers were removed and each rat received 30 pellets individually delivered at random times throughout the 30-min. session. During the next 2 days the levers were present and each lever press was reinforced. On these 2 days each rat was removed after it had responded 50 times or after 30 min. had elapsed. All further daily sessions lasted for 30 min. During the next six sessions the rats were allowed to respond for pellets, which were delivered according to a variable interval 1-min. schedule (i.e., a lever press was reinforced on the average of one time per minute).

The next 5 days constituted the preexposure phase of the experiment. This phase was conducted with the lever retracted. For this phase and the remainder of the experiment the rats were divided into four groups of eight rats. Groups Noise-Shock/CER and Noise-Shock/Punishment were preexposed to uncorrelated presentations of the noise and shocks, whereas Groups CER and Punishment were placed in the boxes each session but received no noise or shock presentations. The uncorrelated procedure consisted of six shocks occurring at irregular intervals each session and two 3-min. noise presentations commencing on the 9th and 21st minute of each session. The presentation of shocks was governed by two constraints. Two shocks never occurred within less than 10 sec of one another and one shock occurred during a noise trial each day.

Following the preexposure phase was a lever-press recovery day during which the lever was present and the animals were permitted to respond on the variable interval food schedule. The final 5 days of the experiment comprised the test phase. During this phase, Groups Noise-Shock/CER and CER received the CER test procedure and Groups Noise-Shock/Punishment and Punishment received the punishment test. For the punishment and CER tests, two 3-min. noise trials beginning on the 9th and 21st minute of each session were given. During each of these trials for both tests, three shocks were programmed. In the CER test the shocks occurred immediately when programmed. However, for the punishment test, the shocks did not occur until after the next response during a noise trial. During this phase the response rate during each trial (CS score) and during the 3 min. immediately preceding each trial (pre-CS score) was recorded. During the recovery day, two "mock Pre-CS scores" were recorded during the periods in which they would have occurred if the CS had been presented. Suppression was measured using Kamin's suppression ratio (cf. Annau & Kamin, 1961), which is simply the proportion of the total responses occurring in both pre-CS and CS periods that occur in the CS period.

Rodger's (1967) method was chosen as the method of analysis. By this method analyses of variance and orthogonal a posteriori comparisons are calculated in a manner identical to that proposed by Scheffé (1959). These variance ratios, however, are compared against Rodger's statistic (Rodger, 1975) rather than Fisher's (cf. Winer, 1971). Choice of this statistic implies choice of a decisionwise error rate rather than an experimentwise one and thereby keeps the power to detect differences constant in experiments containing various numbers of groups.

Results

During the shaping and variable interval training sessions all of the animals developed adequate rates of responding. On the last day of variable interval training the groups' response rates varied from 30 to 35 responses per minute and did not differ reliably, $F(3, 28) = .84$, $p \geq .10$. However, after the preexposure phase, during the mock pre-CS trial on the lever press recovery day, the groups' mean response rates did differ reliably, $F(3, 28) = 23.8$, $p \leq .05$. A posteriori tests indicated that this difference was due to Group CER responding at a faster rate (48 responses/min.) than the other groups (range: 24–27 responses/min.), $F(3, 28) = 2.28$, $p \geq .05$.

The daily mean suppression ratios are plotted in Fig. 1. Analyses of variance were computed for each day of the test. These ratios were all significant, Day 1 $F = 12.71$, $p \leq .01$; Day 2 $F = 4.71$, $p \leq .01$; Day 3 $F = 5.32$, $p \leq .01$; Day 4 $F = 4.44$, $p \leq .01$; Day 5 $F = 6.52$, $p \leq .01$ (all $df = 3, 28$). A posteriori analyses indicated that on Day 1, Groups Noise-Shock/CER and Noise-Shock/Punishment were reliably less suppressed than Groups Punishment and CER, $F(3, 28) = 12.71$, $p \leq .01$. On Days 2–4, Group Noise-Shock/CER was less suppressed than the other groups, Day 2 $F = 4.26$, $p \leq .01$; Day 3 $F = 4.90$, $p \leq .01$; Day 4 $F = 3.75$, $p \leq .01$ (all $df = 3, 28$). Finally, on Day 5, Group Noise-Shock/Punishment was more suppressed than Group Noise-Shock/CER, $F(3, 28) = 3.10$, $p \leq .05$, and Group Punishment was more suppressed than Group CER, $F(3, 28) = 2.37$, $p \leq .05$.

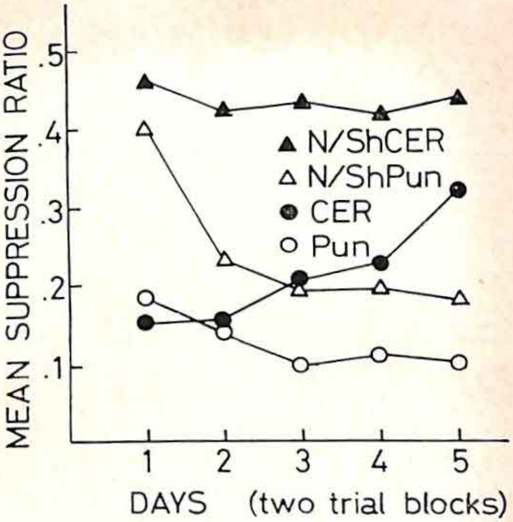

FIGURE 1. Daily mean suppression ratios for the test phase of Experiment 1. (In this and the succeeding experiments the following conventions were used: The shape of symbol [i.e., triangle or circle] represents the stimuli occurring during preexposure. If the symbols are connected with a solid line, preexposure was carried while the animals were not responding. A broken line indicates that the animals were responding during preexposure. Closed symbols represent a CER test, whereas open symbols represent a signaled punishment test. Abbreviations: N = noise; S = shock; CER = conditioned emotional response; Pun = punishment.)

It is possible that some of the differences reported here and in later experiments are due not to differing response rates during the CS but to differences in overall baseline suppression between the groups. To test this, response rate during the pre-CS periods was used as an indicator of overall suppression. A split-plot analysis of variance was carried out on the pre-CS scores of each experiment. In the present case, although Group CER had a mean pre-CS rate of 41 responses/min. and the rates of the other groups varied from 29 to 33 responses/min., there was no reliable treatments main effect, $F(3, 28) = .87$, $p \geq .10$. There was a significant days effect, $F(4, 112) = 1.66$, $p \leq .05$, which was due to a monotonic increase in mean response rate from 31 to 38 responses/min. over the five test sessions. The interaction term was also significant, $F(12, 112) = 1.32$, $p \leq .05$. This difference was due to the fact that on Day 1, Group Punishment (24 responses/min.) responded more slowly than Group CER (41 responses/min.), $F(3, 28) = 2.03$, $p \leq .05$, and on Day 4, Group CER (51 responses/min.) responded more rapidly than the other groups (range: 28–33 responses/min.), $F(3, 28) = 2.26$, $p \leq .05$.

Discussion

The object of this experiment was to demonstrate that uncorrelated presentations of the CS and US could interfere with the acquisition of both CER and signaled punishment suppression. The present results demonstrate just this. It is clear from Figure 1 that the two groups preexposed to uncorrelated presentations of the CS and US (Groups Noise-Shock/CER and Noise-Shock/Punishment) were initially less suppressed than the appropriate control groups (CER and Punishment, respectively). These differences cannot be explained by the suggestion of Weiss et al. (1968) that exposure to unpredictable shock causes conditioned freezing. Any such effect could only have increased freezing during the test phase.

On the final day of the test, Group Noise-Shock/Punishment was more suppressed than Group Noise-Shock/CER, and Group Punishment was more suppressed than Group CER. These results confirm the results of Church et al. (1970) that the present signaled punishment procedure causes more suppression than the CER procedure. This difference is presumably due to the response-shock contingency present in signaled punishment but not in CER. However, the observation that this difference in suppression developed earlier in the preexposed groups than in the nonpreexposed groups may shed some light on the role of the two contingencies which operate in signaled punishment situations. It is possible that in many signaled punishment situations much of the suppression observed is due not to the response-shock contingency but to the fact that these shocks occur only during the CS. To the extent that this is true, both signaled punishment and CER produce suppression for the same reason—because of the contingency between CS and shock. This is consistent with the present observation that Groups Punishment and CER showed equal amounts of suppression during Days 1 and 2 of the test phase. During this time, suppression may have been largely due to the contingency between noise and shock and only later may the response-shock contingency have controlled suppression in Group Punishment, thus causing the separation between the two groups over Days 3–5.

If this is so, then it is possible that the major effect of preexposure to uncorrelated noise and shock in the noise-shock/punishment and noise-shock/CER groups may have been to interfere with the learning of the classical contingency between noise and shock. There is no particular reason to suppose that preexposure interfered with the learning of the instrumental response–shock contingency in Group Noise-Shock/Punishment. Indeed, the observation that this group separated from Group Noise-Shock/CER sooner than Group Punishment separated from Group CER suggests that the noise-shock/punishment group learned the response–shock contingency at least as fast as its nonpreexposed control group. By using an instrumental procedure that has certain similarities to a classical procedure, it becomes possible that any effects observed in the instrumental procedure are due to the classical components it contains.

EXPERIMENT 2

Although establishing that preexposure to uncorrelated signals and shocks may interfere with the acquisition of suppression in the signaled punishment procedure, Experiment 1 could not show that this effect reflected any interference with the learning of the response–shock contingency inherent in that procedure. As a preliminary step toward establishing whether such an effect could be obtained in signaled punishment, Experiment 2 provided a more extensive comparison of the effects of various preexposure treatments on the subsequent acquisition of punishment suppression. One group was preexposed to uncorrelated presentations of the noise and shocks, another group was preexposed to the shocks, a third group received noise trials only, and the fourth group received no programmed stimuli. A major difference between Experiments 1 and 2 was that in the latter case preexposure was carried out while the animals were pressing the levers for food.

Method

Subjects and apparatus. Thirty-two hooded male Long-Evans rats, obtained and maintained as in Experiment 1, were used. The conditioning chambers and stimuli described in Experiment 1 were used. Shocks were .4 mA in intensity and .5 sec in duration.

Procedure. The subjects were trained to press the lever and given 6 days of variable interval training, in a manner similar to Experiment 1. For the preexposure phase, which lasted for 5 days, the animals were divided into four groups. The noise/shock group received the uncorrelated treatment described for Experiment 1. A second group received the same treatment with the noise trials omitted, the third group received the same treatment with shocks omitted, and the fourth group received no programmed stimuli. For all conditions, the lever remained in the box and the animals were allowed to continue responding for food.

The next day was a lever-press recovery day during which no stimuli occurred. The next 5 days constituted the test phase during which all of the animals received the signaled punishment test procedure described for Experiment 1.

Results

The rats learned to press the lever and over the variable interval training period established a rate of variable interval responding that was similar to that in Experiment 1. Their response rates did not differ on the last day of variable interval training, $F(3,\ 28)\ =\ 1.88,\ p\ \geq\ .10$, although the noise group responded at a rate of 47 responses/min., whereas the other groups' rates ranged from 27 to 31 responses/min. On the bar-press recovery day, the shock group, with a rate of 22 response/min., was significantly, $F(3, 28) = 2.07$, $p \leq .05$, more suppressed than the other groups, whose rates varied from 40 to 42 responses/min. However, on the first pre-CS trial of the test this group responded at a rate of 36 responses/min., which was within the range of the other groups' rates (range = 31 to 42 responses/min.), $F(3, 28) = .45$, $p \geq .10$.

Figure 2 depicts the mean daily suppression ratios for each day of the test. One-way analyses of variance were calculated for each day and they were all significant, Day 1 $F = 4.58$, $p \leq .01$; Day 2 $F = 3.52$, $p \leq .01$; Day 3 $F = 2.75$, $p \leq .05$; Day 4 $F = 2.91$, $p \leq .05$; Day 5 $F = 2.91$, $p \leq .05$ (all $df = 3, 28$). A posteriori analyses indicated that on Day 1 the noise/shock group was less suppressed than the other groups, $F(3, 28) = 4.36$, $p \leq .01$, while on the remaining days the noise/shock and shock groups were

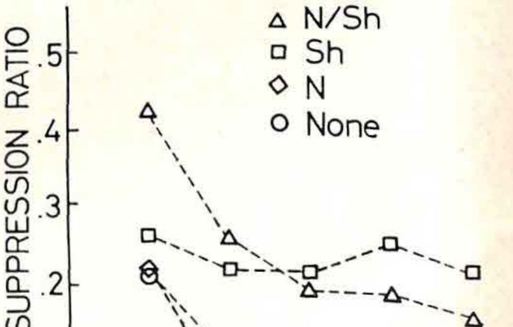

FIGURE 2. Daily mean suppression ratios for the test trials of Experiment 2. (N = noise; S = shock.)

less suppressed than the noise and no-stimuli groups, Day 2 $F(3, 28) = 3.23$, $p \leq .05$; Day 3 $F = 2.60$, $p \leq .05$, Day 4 $F = 2.67$, $p \leq .05$, Day 5 $F = 2.62$, $p \leq .05$.

A split-plot analysis of variance was carried out on the mean daily pre-CS scores for the test phase. The overall mean response rates were 48 responses/min. for the noise/shock group, 32 responses/min. for the shock group, 43 responses/min. for the noise group, and 31 responses/min. for the no-stimuli group. These rates did not differ reliably, $F(3, 28) = 1.16$, $p \geq .10$. The overall mean daily rates increased from 33 to 38 responses/min., $F(4, 112) = 4.03$, $p \leq .01$. On the first test day, the no-stimuli group, with a rate of 20 responses/min., was less suppressed than the other groups, whose rates varied from 33 to 38 responses/min. On the final 2 days of the test, the noise/shock and noise groups, whose mean rates were 54 and 53 responses/min., responded more often than the shock and no-stimuli groups, both of which responded at 31 responses/min. These differences account for the reliable Treatments × Days interaction, $F(12, 112) = 3.04$, $p \leq .01$. The former difference may well account for the fact that preexposure to the noise caused no apparent interference with the acquisition of suppression. In fact, the response rate of the noise group was 27 responses/min. during the first CS period of the test, while that of the no-stimuli group was 15 responses/min. These rates differ reliably, $t(14) = 2.18$, $p \leq .05$. The difference between the noise/shock and noise groups and the shock and no-stimuli groups on the final 2 days of testing appears to have had no effect on the groups' relative suppression, as measured by suppression ratios. This is because, as inspection of Figure 1 indicates, suppression of all groups remained stable on the final 4 days of the test.

Discussion

The results of this experiment are that the noise/shock group was less suppressed than the remaining groups on the first day of the test and that on all subsequent days the noise/shock and shock groups, though not differing from each other, were less suppressed than the noise and no-stimuli groups. These results replicate those of Experiment 1, in which it was found that preexposure to uncorrelated noises and shocks interfered with the acquisition of punishment suppression. They also demonstrate that preexposure to shock alone, at least while responding, is sufficient to cause interference with the acquisition of punishment suppression. Finally, there was no evidence that preexposure to noise alone had any effect on test performance, but it is possible that this failure to observe latent inhibition (Lubow & Moore, 1959) was due to the fact that the appropriate comparison group, no-stimuli, responded slowly on the first test day.

The difference between the noise/shock and shock groups on the first test day suggests either an effect of latent inhibition produced by exposure to the noise alone or an effect of learned irrelevance produced by exposure to uncorrelated presentations of noise and shock. In either case, it is reasonable to suggest that the difference reflects a failure of the classical contingency between noise and shock to affect subjects in the noise/shock group on early test trials. Thus, the initial suppression in the shock group may have been largely due to the effects of this classical contingency—a conclusion consistent with the argument advanced in the discussion of Experiment 1.

The most important feature of the present results, however, is that after the first day, exposure to shock alone interfered as much with the acquisition of suppression as did exposure to uncorrelated noise and shock. The discussion of Experiment 1 suggested that after one or two days of a signaled punishment procedure, the instrumental contingency between responding and shock became the most important determinant of suppression. If this is true, the implication is that exposure to shock alone was sufficient to interfere with the effects of this instrumental contingency. This is consistent with the fact that Mackintosh (1973) found little or no effect of exposure to shock alone on the acquisition of conditioned suppression in a CER paradigm. Exposure to shock may affect suppression produced by an instrumental contingency without affecting suppression produced by a classical contingency.

In the present context this seems a reasonable suggestion since in Experiment 2 animals were exposed to shock while continuing to lever press on a variable interval schedule. Therefore, both the noise/shock and shock groups had the opportunity to learn that lever pressing was uncorrelated with the delivery of shock. If learned irrelevance is a consequence of learning that a particular CS and US are uncorrelated, then it is possible that at least one component of learned helplessness (there may, of course, be more general components) is a consequence of learning that a particular response and reinforcer are uncorrelated. Experiment 3, therefore, was designed to discover whether the effect of preexposure to uncorrelated noise and shock on the acquisition of punishment suppression depended on whether or not preexposure was conducted while animals continued to lever press.

EXPERIMENT 3

This experiment was carried out to attempt to demonstrate that rats preexposed to the uncorrelated procedure while responding (Group On) and hence able to learn that responding and shocks are uncorrelated would suppress less in a signaled punishment test than rats also preexposed to the uncorrelated procedure but without the opportunity to respond (Group Off).

Method

Subjects and apparatus. Twenty-four hooded male Long-Evans rats, obtained and maintained in a similar manner to Experiment 1, were used. The conditioning chambers and stimuli described for Experiment 1 were used. Shocks were .25 mA in intensity and lasted for .5 sec.

Procedure. The rats were shaped to lever press and given 7 days of variable interval lever-press training in a similar manner to Experiment 1. All sessions except the shaping sessions were 30 min. in duration.

The rats were divided into two equal groups for the preexposure phase, which lasted 5 days. During this phase, both groups of animals received the uncorrelated procedure described in Experiment 1. Group On was allowed to continue responding for food, but for Group Off the lever was removed and hence they could not respond. After the preexposure phase came a lever-press recovery day, during which two nonreinforced noise trials occurred. The final 5 days comprised the test phase, during which both groups received the punishment test described for Experiment 1 (i.e., three response-contingent shocks programmed to occur in each of two 3-min. noise trials per day).

Results

Groups On and Off behaved similarly during shaping and variable interval training. On the last day of training both groups had mean response rates of 28 responses/min., $t(22) = .024$, $p \geq .10$. On the lever-press recovery day, Group On responded at 23 responses/min., while Group Off responded at 29 responses/min. These rates did not differ reliably, $t(22) = 1.35$, $p \geq .10$. During the first pre-CS period of the test, Groups On and Off responded at 34 and 30 responses/min. These rates did not differ reliably, $t(22) = .29$, $p \geq .10$. Groups On and Off had suppression ratios of .53 and .51 to the noise on the lever-press recovery day. These ratios did not differ reliably, $t(22) = .57$, $p \geq .10$.

The results of the test phase are shown in Figure 3. A split-plot analysis of variance was carried out on the test suppression

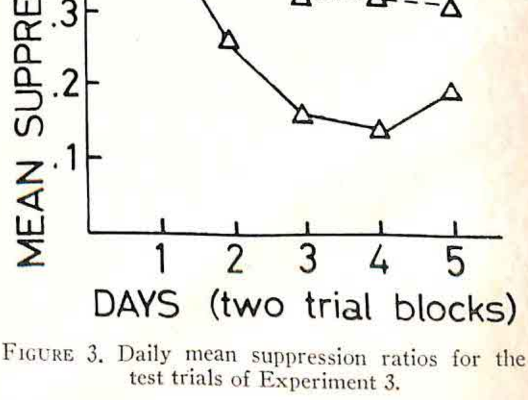

FIGURE 3. Daily mean suppression ratios for the test trials of Experiment 3.

ratios. The main effect for treatments was significant, $F(1, 22) = 4.99$, $p \leq .05$. This factor indicates an overall difference in suppression between Groups On and Off. The main effect for days was also significant, $F(4, 88) = 25.25$, $p \leq .01$. This factor indicates an increase in suppression over the test phase. Finally, the groups did not differ on the first test day but did on the others. This result accounts for the significant Days × Treatments interaction in the analysis, $F(4, 88) = 3.04$, $p \leq .01$.

A split-plot analysis of variance was carried out on the pre-CS scores. The overall mean rate of Group On was 35 responses/min. and that of Group Off was 28 responses/min. These rates did not differ reliably, $F(1, 22) = .83$, $p \geq .10$. The mean daily response rates increased from 29 to 34 responses/min. but did not differ reliably, $F(4, 88) = 1.56$, $p \geq .05$. The interaction term of the analysis was also not significant, $F(4, 88) = .98$, $p \geq .10$.

Discussion

The results of this experiment demonstrate that uncorrelated presentations of noises and shocks interfere more with the acquisition of punishment suppression if the animal is permitted to emit the to-be-punished response than if not. The contention tested in this experiment was that the animals in Group On would learn that shocks and lever pressing were unrelated during preexposure and that this learning would subsequently interfere with the acquisition of punishment suppression. The results clearly are consistent with this contention; however, they are open to alternative interpretations.

If it is to be claimed that the interference shown by Group On was due to learning that lever pressing and shocks are uncorrelated, and not to some more general effect of preexposure while responding, then it must be shown that the interference effect is specific to the punishment contingency. In the next experiment it was endeavored to show that preexposure to uncorrelated noise and shocks while responding would interfere more with the acquisition of punishment suppression than preexposure while not responding but that both preexposure procedures would have a similar effect on CER suppression. Such a demonstration would strongly support the contention that the effect of the present interference was on the punishment contingency.

EXPERIMENT 4

In this experiment two groups were preexposed to uncorrelated noise and shocks while lever pressing and two groups were preexposed while not lever pressing. One group preexposed while responding (Group On-Punishment) and one group preexposed while not responding (Group Off-Punishment) received the signaled punishment test. The other group preexposed while responding (Group On-CER) and the other preexposed while not responding (Group Off-CER) received the CER test.

Method

Subjects and apparatus. The subjects were 32 male Long-Evans rats obtained and maintained in the same manner as in Experiment 1. The same apparatus and stimuli as described in Experiment 1 were used except that the shocks were .4 mA in intensity.

Procedure. The rats were trained to lever press and given 6 days of variable interval training using the method described for Experiment 1. For the test phase, which lasted 5 sessions, the animals were divided into four groups of eight each. Two groups were preexposed to the uncorrelated procedure described for Experiment 1 while responding for food, and two groups were preexposed to uncorrelated procedure but were not allowed to respond for food because the levers were not present. The next day was a lever-press recovery day during which both groups were allowed to respond for food and during which two mock pre-CS scores were recorded. The next 3 days comprised the test phase. During this phase one group of animals (Group On-CER) which had been preexposed while responding and one group of animals which had been preexposed while the lever was removed (Group Off-CER) received the CER test described in Experiment 1. One group of animals (Group On-Punishment) preexposed while responding and one group preexposed while not responding (Group Off-Punishment) received the punishment test described in Experiment 1.

Results

The animals learned to press the lever, and on the last day of variable interval training the groups' mean response rates varied from 27 to 36 responses/min. and did not differ significantly, $F(3,28) = .19$, $p \geq .10$. On the recovery day Group Off-Punishment responded at 36 responses/min., Group On-Punishment responded at 36 responses/min., Group On-CER at 46 responses/min., and Group Off-CER at 55 responses/min. These rates did not differ significantly, $F(3, 28) = .80$, $p \geq .10$.

The mean daily suppression ratios for the test are shown in Figure 4. One-way analyses of variance indicated that these scores did not differ reliably on the first 2 test days but did on the third, Day 1 $F = .51$, $p \geq .10$, Day 2 $F = 1.85$, $p \geq .05$, Day 3 $F = 2.61$, $p \leq .05$ (all $df = 3, 28$). A posteriori analysis indicated that Group Off-Punishment was more suppressed than the other groups on the third test day, $F(3, 28) = 2.36$, $p \leq .05$. A split-plot analysis of variance was carried out on the pre-CS scores during testing. Although Groups On-CER (40 responses/min.) and Off-CER (44 responses/min.) responded at higher rates than Group Off-Punishment (31 responses/min.) or On-Punishment (22 responses/min.), the main effect for treatments was not significant, $F(3, 28) = 1.76$, $p \geq .10$. Inspection of the raw data indicates that the differences were due to one animal each in Group On-CER and Group Off-CER responding consistently at a very high rate (> 100 responses/min.). The mean daily response rates for all groups increased from 30 to 36 responses/min. This increase was reliable, $F(2, 56) = 4.61$, $p \leq .01$. There was no reliable interaction, $F(6, 56) = .91$, $p \geq .10$.

Discussion

The results of this experiment support the hypothesis that preexposure to uncorrelated noises and shocks while responding interferes with the learning of the punishment contingency in a signaled punishment test. Such a hypothesis predicts that rats so preexposed would suppress more slowly during a signaled punishment test than rats preexposed while not lever pressing. However, preexposure to noise and shocks should have an equal effect on CER suppression regardless of whether it is carried out while responding. The suppression scores in Figure 4 support this hypothesis. Group On-Punishment was less suppressed on the later test trials than Group Off-Punishment. On the other hand, there was no difference in suppression between Groups On-CER and Off-CER. In fact, throughout this period Group On-CER exhibited nonsignificantly more suppression than Group Off-CER.

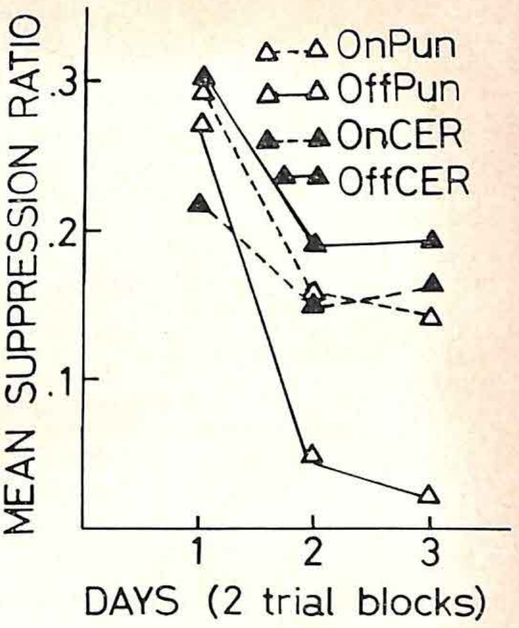

FIGURE 4. Daily mean suppression ratios for the test trials of Experiment 4. (Pun = punishment; CER = conditioned emotional response.)

Another way of regarding the test scores of Experiment 4 also supports this contention. This hypothesis states that animals can learn that responses and reinforcers are uncorrelated and that because of this they will learn future contingencies more slowly than controls. If a rat is placed in a signaled punishment situation and fails to learn the response-shock contingency, then the situation should, to the rat at least, approximate a CER test. If such is the case, then rats preexposed while responding should behave similarly on both a CER and a signaled punishment test. It is apparent from Figure 4 that the rats in Groups On-CER and On-Punishment were similarly suppressed whereas those in Group Off-Punishment were more suppressed than those in Group Off-CER. These results clearly support the contention that Group On-Punishment was retarded in learning the punishment contingency, and Group Off-Punishment learned in a typical manner. This learning in Group Off-Punishment followed a similar course to that found in Experiment 1 with a milder shock (compare Group Noise-Shock/Punishment and Group Noise-Shock/CER).

General Discussion

The present series of experiments has demonstrated that preexposure to uncorrelated presentations of noises and shocks when rats are not permitted to respond interferes with the acquisition of both CER and signaled punishment suppression. If the rats are permitted to continue responding during preexposure, there is additional retardation of the acquisition of signaled punishment suppression but not CER suppression. The first result is consistent with Mackintosh's (1973) learned irrelevance results, and the second is consistent with Maier, Seligman, and Solomon's (1969) learned helplessness results. It seems plausible to suggest that both the present results and those of the above studies can be explained by similar mechanisms. In the case of CER suppression and the CER component of signaled punishment suppression, the suggestion is that the animal preexposed to the uncorrelated procedure learns that the CS and shocks are uncorrelated, and this interferes with learning when the CS and shocks are later made positively correlated. Similarly, in the case of signaled punishment, an animal preexposed to shocks while responding learns that the shocks and its responding are uncorrelated, and this interferes with learning when the shocks and responding are later made correlated (i.e., the punishment contingency is introduced).

On the surface, these results imply that rats must be considered as correlation learners. A modification of associative-learning models that might allow them to come to grips with the present problem would be to assume that the learning rate parameters for the CS and US are variable rather than constant (see e.g., Wagner & Rescorla, 1972). Such a modification would allow various schedules of preexposure to modify the value of these parameters. For instance, if preexposure to the random procedure were to lower the parameters, than slower learning (learned irrelevance) would result. A similar analysis is applicable to the learned helplessness results. Conversely, some treatments might increase rather than decrease these learning rate parameters. For instance, learning a positive correlation might increase the learning rate parameter of the US and lead to future "positive transfer."

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ERRATUM

In the article "Sources of Retroactive Inhibition in Pigeon Short-Term Memory" by Douglas S. Grant and William A. Roberts, which appeared in the *Journal of Experimental Psychology: Animal Behavior Processes* (Vol. 2, No. 1, January 1976, pp. 1–16), the word *not* was omitted from the first sentence in the first paragraph on page 14. The sentence should read "That interpolated illumination has powerful effects upon short-term retention in the pigeon perhaps should *not* be surprising, since a number of experiments with primates have revealed similar effects" (italics added).

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Effects of Flooding an Irrelevant Response on the Extinction of Avoidance Responses

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Three experiments are reported concerning the transsituational effects of response prevention (flooding) in the extinction of avoidance responding. Experiment 1 demonstrated that flooding a jump-up avoidance response can hasten the extinction of a shuttlebox avoidance response learned to a very different conditioned stimulus (CS). Experiment 2 demonstrated that an irrelevant fear extinction experience can also hasten the extinction of a shuttlebox avoidance response. This supports the hypothesis that Pavlovian generalization of extinction of conditioned fear across CS modalities mediates the irrelevant flooding effect. Experiment 3 showed that flooding a shuttlebox avoidance response does not hasten the extinction of a jump-up avoidance response learned to a different CS. Possible reasons for the asymmetry of the irrelevant flooding effect are discussed.

Flooding, or response-prevention, techniques are effective in producing rapid extinction of well-learned avoidance responses, even though such responses are highly resistant to extinction with conventional procedures. These techniques use prolonged exposure to the conditioned stimulus (CS), either with the avoidance response forcibly prevented or with CS termination noncontingent on the response. Over the last 15 years, two different types of theories have emerged to explain the efficacy of flooding in hastening the extinction of avoidance responses. These theories differ principally in what they consider to be the crucial learning process that occurs during flooding.

One type of theory asserts that flooding, by allowing for extended nonreinforced exposure to the CS, reduces fear and thus eliminates both the motivation and the reinforcement for continued responding. Two-process theory in its various forms (e.g. Mowrer, 1947; Rescorla & Solomon, 1967; Solomon & Wynne, 1954) and Baum's relaxation theory (1970) fit into this category. Whereas two-process theory assumes fear has extinguished following flooding, Baum's theory emphasizes the acquisition of relaxation responses in the return to a nonfearful behavioral repetoire. The evidence suggests, however, that this parsimonious explanation is not completely adequate. A number of investigators have noted that fear of the CS, as indexed by a passive avoidance test, remains after flooding a one-way avoidance response (Coulter, Riccio, & Page, 1969; Linton, Riccio, Rohrbaugh, & Page, 1970; Page, 1955).

The second type of theory assumes that some fear may remain following flooding but that the avoidance operant extinguishes because the animal has learned some new way to respond in the presence of fear. Page (1955), Coulter et al. (1969), and Linton et al. (1970) have argued that during flooding a competing response is adventiously re-

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inforced in the presence of the CS. This competing response comes to replace the avoidance response during extinction. In their cognitive theory of avoidance learning, Seligman and Johnston (1973) argued that flooding hastens extinction of the avoidance response because it produces a change in the act-outcome expectancies governing an animal's responding: The animal's expectancy that not responding will lead to shock is disconfirmed, and the more disconfirmation he receives, the faster he should stop responding. According to this account, fear extinction is neither necessary nor sufficient for extinction of the avoidance response to occur; rather, a change in act-outcome expectancies is necessary for the avoidance response to extinguish. Thus both the competing-response account and the cognitive account assume that fear of the CS may remain after flooding, but the response made in the presence of fear changes.

None of these accounts of flooding make any precise predictions about the specificity of the effects of flooding, that is, whether flooding one avoidance response will hasten the extinction of a second avoidance response learned to a different CS. Yet, a recent study by Watson and Marks (1971) on flooding therapy for humans with severe phobias raised the definite possibility that flooding could have such transsituational effects. These investigators found that patients who were flooded with scenes totally irrelevant to their specific phobia (scenes which in fact any normal human would react to with great fear or anxiety) showed as much improvement on their specific phobia as did patients flooded on scenes relevant to their specific phobia. Although avoidance responses in animals are sometimes considered to be a poor model for human phobias (e.g. Costello, 1970; Seligman, 1971), the flooding therapy used with human phobias has been frequently and profitably compared with flooding or response-prevention techniques used to extinguish animal avoidance responses (Baum, 1970; Marks, 1972).

The present experiments explored the specificity of the effects of flooding, by determining if flooding an avoidance response learned to one CS would have transsituational effects, that is, would hasten the extinction of a different avoidance response learned to a different CS.

EXPERIMENT 1

Experiment 1 compared the effects of relevant and irrelevant flooding treatments on the extinction of a shuttlebox avoidance response. All groups were trained in both the jump-up box and the shuttlebox so as to equate past learning experience at the time of testing.

Method

Subjects

Subjects were 50 male albino rats of the Fischer strain obtained from Microbiological Associates, Walkersville, Md. They were 90–120 days old at the time of the experiment, housed individually, and maintained on ad-lib food and water.

Apparatus

Two different avoidance conditioning chambers composed the apparatus for this experiment: (a) an automated, one-way jump-up box, similar to the one described by Baum (1965) and (b) an automated, two-way shuttlebox.

Jump-up box. The box measured 30.5 × 30.5 × 35.6 cm and was constructed of plywood painted black except for the front which was Plexiglas. The grid floor consisted of 15 stainless steel rods through which 1-mA constant current shock could be delivered. A ledge, 17.1 cm above the grid floor and protruding 10.2 cm into one side of the box, rested and slided on two Plexiglas runners which were mounted to the front and back sides of the box. Each runner was slit in the middle to allow backward–forward movement of the ledge into and out of the box. A Redmond motor (Type AF4, 1650 rpm) automatically pulled the ledge out of the box via a fishline, and two springs served to pull the ledge back into the box. Retraction and reinsertion time was approximately 1 sec. A photocell detected the presence of the rat on the ledge. A Jensen speaker mounted on the outside of the box delivered a 1,300-Hz tone (90 db. against a background of 65 db.) which served as the CS. All programming was done automatically through standard relay programming and recording equipment.

Shuttlebox. The automated, two-way shuttlebox measured 42.5 × 13.6 × 22.2 cm and was constructed of sheet metal and Plexiglas. The grid floor consisted of 18 stainless steel rods mounted on a Plexiglas frame that was divided and hinged in the middle. Microswitches mounted near each end served to detect the position of the animal. Two .10-A, 28-V miniature lamp bulbs were mounted above the Plexiglas ceiling and served as the CS. One mA constant current shock delivered to the grid floor served as the unconditioned stimulus (UCS). A stainless steel bar mounted 2.5 cm above the grid served as the hurdle. The shuttlebox was enclosed in a larger sound-attenuating enclosure. Again all stimuli were automatically presented and responses recorded by standard relay programming equipment. White masking noise was used throughout all experimental sessions in both the jump-up box and the shuttlebox.

Procedure

In all, four groups of rats were run, with two groups of 15 and two groups of 10. The experiment was divided itno six phases over the course of 2 days.

Day 1. All four groups of rats received avoidance acquisition training in both the jump-up box and the two-way shuttlebox. Half of the subjects were trained in the jump-up box first and the remainder in the shuttlebox first.

Jump-up box. Each subject was placed on the ledge which started a 50-sec interstimulus interval (ITI). Five seconds before the end of the ITI, the 1300-Hz tone CS came on. At the end of this 5-sec CS, the ledge was retracted automatically (for less than a second), thus dropping the rat onto the grid floor. Ten seconds after landing on the grid floor, the grid was electrified until the rat escaped by jumping or climbing back onto the safety ledge. The rat was allowed to remain on the ledge for 50 sec (ITI) before being dropped onto the grid floor again, thus starting the next trial. The 5-sec CS always preceded the retraction of the ledge. The rat was able to avoid shock by jumping back onto the ledge within 10 sec of being dropped on the floor. Each rat was trained to a criterion of 10 consecutive avoidance responses.

Shuttlebox. Each rat was placed in the shuttlebox and allowed 1–2 min to habituate to it before the first light CS was presented. Five seconds after the light came on, the grid floor was electrified until the rat had jumped to the other side of the shuttlebox. The light terminated with the response. The trials were presented on a variable time (VT) 1-min schedule. On any trial the rat could avoid shock by jumping to the other side of the box during the first 5 sec of the CS. Each rat was trained to a criterion of 10 consecutive avoidance responses. Any subject failing to reach criterion in the first 100 trials on Day 1 was given a second day of acquisition before testing on a third day.

Day 2. On Day 2, each group of rats received a different sequence of training, treatment, and extinction trials. *The acquisition criterion in each box was always 10 consecutive avoidance responses.* Table 1 clarifies these procedural differences for the four groups, as well as presenting the acquisition and extinction data for each group.

Relevant Flood group ($n = 10$). Each rat was first given reacquisition training in the jump-up box, with the same procedure as described for Day 1. Immediately after reaching criterion, the subject was placed in the shuttlebox where reacquisition training occurred. Upon reaching criterion, the rat was prevented from crossing the midline by a metal barrier placed in the middle of the shuttlebox. Subsequently, 20 flooding trials were given on the same VT 1-min schedule. A flooding trial consisted of a 5-sec CS with no response allowed. No shocks were given. Following the 20 flooding trials, the subject was removed from the apparatus for 1 min, allowing time for the experimenter to remove the barrier. The subject was then replaced in the apparatus and extinction trials began. During extinction the 5-sec CS was presented for 40 trials on the VT 1-min schedule. A response would terminate the CS, but if no response was made, the CS terminated automatically after 5 sec. No shocks were given in this phase.

Irrelevant Flood group ($n = 15$). Each rat was first given reacquisition training in the shuttlebox. The procedure was identical to that on Day 1. Upon reaching criterion, the rat was removed from the shuttlebox and placed in the jump-up box where reacquisition training began. After the subject had reached criterion in the jump-up box, the shock was turned off and the ledge was manually disconnected by the experimenter so that the irrelevant flooding session could begin. After the last ITI the subject was dropped onto the floor, but not permitted to jump back onto the ledge since the ledge remained retracted. Flooding consisted of remaining on the grid floor for 20 min with the 5-sec tone being presented once a minute (20 times). Upon completion of the irrelevant flooding treatment, the subject was removed from the jump-up box and replaced in the shuttlebox where extinction trials began. Extinction consisted of 40 CS presentations during which a response would terminate the CS, but if no response was made the CS terminated automatically at the end of 5 sec.

Jump-Up Control group ($n = 15$). The procedure for this group was identical to that for the Irrelevant Flood group, except that instead of receiving 20 flooding trials in the jump-up box after reaching criterion there, subjects in this group received 20 more acquisition trials in the jump-up box. Following this continued acquisition training, the subject was placed in the shuttlebox for 40 extinction trials. This group was run with continued acquisition training in the jump-up box, to control approximately for the time the Irrelevant Flood group had spent in the jump-up box prior to shuttlebox extinction.

Home Cage Control group ($n = 10$). The procedure for this group was identical to that for the Relevant Flood group, except that instead of receiving 20 flooding trials in the shuttlebox prior to extinction, they spent 30 min in their home cages. After jump-up reacquisition, followed by shuttlebox reacquisition, each subject was returned to his home cage for 30 min. Following this 30-min period, each rat was returned to the shuttlebox where it received 40 extinction trials. This group was run with 30 min in the home cage prior to shuttlebox extinction, to control for the time spent between shuttlebox acquisition and extinction both for the Irrelevant Flood group and the Relevant Flood group. Twenty minutes in the home cage would have been the operationally correct control group for the Relevant Flood group, but since 30 min was biased in the conservative direction it was deemed appropriate to use as a control procedure for both the Relevant and Irrelevant Flood groups.

Results

Acquisition and Reacquisition

The number of trials to criterion and the number of shocks received for each group are depicted in Table 1. One subject's data from the Irrelevant Flood group were excluded from the data analysis due to equipment problems during testing. On Day 1, the Irrelevant Flood group tended to take longer to reach criterion and receive more shocks than did the other groups. In the shuttlebox, the Irrelevant Flood group took more trials to criterion and more shocks than did those in the Relevant Flood group (Us = 28 and 24, respectively, $p < .02$, Mann-Whitney U test).[1] No other differences between groups were significant for shuttlebox acquisition. In the jump-up box rats in the Irrelevant Flood group also received significantly more shocks than did those in the Relevant Flood group ($U = 32.5$, $p < .05$), the Home Cage Control group ($U = 24$, $p < .02$), and the Jump-Up Control group ($U = 48.5$, $p < .05$). On Day 2, there were no significant differences across the four groups in trials to criterion or in the number of shocks received.

TABLE 1

Procedure and Acquisition and Extinction Rates in Experiment 1

Group	Procedure
Relevant Flood (n = 10)	
Day 1	Jump-up acquisition (17.6; 3.5) Tone CS
	Shuttlebox acquisition (33.1; 12.8) Light CS
Day 2	Jump-up acquisition (13.4; 2.0) Tone CS
	Shuttlebox acquisition (25.1; 6.9) Light CS
	Shuttlebox flood—20 trials Light CS
	Shuttlebox extinction (9.8) Light CS
Irrelevant Flood (n = 14)	
Day 1	Jump-up acquisition (20.5; 6.1) Tone CS
	Shuttlebox acquisition (67.2; 32.3) Light CS
Day 2	Shuttlebox acquisition (22.6; 5.5) Light CS
	Jump-up acquisition (12.2; 1.1) Tone CS
	Jump-up flood—20 min Tone CS
	Shuttlebox extinction (17.7) Light CS
Jump-Up Control (n = 15)	
Day 1	Jump-up acquisition (16.9; 3.9) Tone CS
	Shuttlebox acquisition (47.7; 17.9) Light CS
Day 2	Shuttlebox acquisition (23.8; 6.0) Light CS
	Jump-up acquisition (12.3; 1.1) Tone CS
	Jump-up continued acquisition—20 trials Tone CS
	Shuttle box extinction (28.1) Light CS
Home Cage Control (n = 10)	
Day 1	Jump-up acquisition (16.3; 3.4) Tone CS
	Shuttlebox acquisition (59.0; 24.8) Light CS
Day 2	Jump-up acquisition (13.4; 1.7) Tone CS
	Shuttlebox acquisition (18.6; 4.1) Light CS
	30 min in home cage
	Shuttlebox extinction (27.0) Light CS

Note. Numbers in parentheses for acquisition represent the average number of trials to criterion and the average number of shocks received, respectively. Numbers in parentheses for extinction represent the average total number of avoidance responses. CS = conditioned stimulus.

The differences between groups during acquisition on Day 1 were judged to be of little importance for interpreting the extinction results following the various flooding and control treatments for two reasons. These differences had disappeared by the second day of acquisiton. In addition, there was no significant correlation, for either the jump-up box or the shuttlebox, between number of trials to criterion on Day 1 or 2 and speed of extinction, or between number of shocks received during acquisition on Day 1 or 2 and speed of extinction, for any of the four groups (31/32 r_ss were *ns*, Spearman rank correlation[2]). Since *within* any group there was no significant correlation between any of these acquisition measures and speed of extinction, the observed differences in acquisition on Day 1 *between* groups were not judged to be important in interpreting the extinction results.

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[1] All tests are two tailed.

[2] The correlation between trials to criterion in the jump-up box on Day 1 and number of avoidance responses in extinction for the Jump-Up Control group was reliable, $r = -.74$, $p < .02$. Due to the large number of correlation coefficients tabulated, this significant one should probably be attributed to chance.

Extinction

Both the Relevant and the Irrelevant Flood groups extinguished faster than the Jump-Up Control and Home Cage Control groups, which did not differ from each other. Kruskal-Wallis one-way analysis of variance for the first 30 trials of extinction revealed a significant difference between flooded and nonflooded groups ($H = 11.47$, $p < .02$). Figure 1 shows the mean number of avoidance responses for each group across four blocks of extinction trials.

The results from extinction were broken down into four blocks of 10 trials each, and the number of avoidance latency responses were counted for each subject for each of these four blocks. As can be seen in Figure 1, differences between the four groups began to lessen by the fourth block of extinction (Trials 31–40). When the first 30 trials are considered together, the Relevant Flood group made fewer avoidance responses than the Home Cage Control group ($U = 17$, $p < .02$) and fewer than the Jump-Up Control group ($U = 27$, $p < .01$). Similarly, the Irrelevant Flood group made fewer avoidance responses than the Home Cage Control group ($U = 37$, $p \approx .06$) and fewer than the Jump-up Control group ($U = 60.5 \approx .06$) and fewer than the two control groups combined ($U = 97.5$, $p \approx .03$). The Relevant and Irrelevant Flood groups did not differ significantly ($U = 44$, $p > .10$), although there was a trend for the Relevant Flood group to extinguish faster. The Jump-Up and Home Cage Control groups did not differ from each other. The same pattern of results emerges when all 40 trials are considered together, although the Irrelevant Flood group made fewer avoidance responses only when the two control groups were combined ($U = 108$, $p \approx .06$).

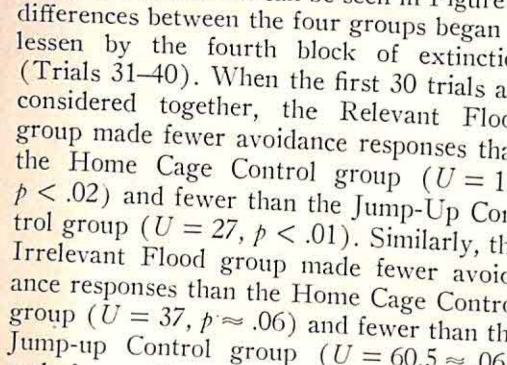

FIGURE 1. Mean number of avoidance latency responses across four blocks of extinction for Relevant Flood, Irrelevant Flood, Home Cage Control, and Jump-Up Control groups of Experiment 1.

Another way of looking at these results is to examine the number of subjects in each group that made more than 30 out of 40 possible avoidance responses in extinction, that is, took the longest to extinguish. Only 10% and 21% of the Relevant and Irrelevant Flood groups made more than 30 out of 40 possible avoidance responses, whereas 53% and 60% of the Jump-Up and Home Cage Control groups met this criterion. This difference is significant on a χ^2 test ($\chi^2 = 8.59$, $p < .05$).

Discussion

The results of Experiment 1 indicate that both relevant and irrelevant flooding can hasten the extinction of a moderately well-learned avoidance response. Although relevant flooding produced a slightly larger decrement in avoidance responding than did irrelevant flooding, this difference was not significant. These results suggest then that flooding can have substantial transsituational effects.

The important question that these results immediately raise is which type of theory of flooding can best account for the irrelevant flooding effect. Specifically, has fear extinction generalized across CS modalities (Pavlov, 1927) to mediate the irrelevant flooding effect? Or has the response to fear changed in one situation, and this change generalized to the other situation? By the first account, extinction of fear to the tone CS that occurs during jump-up box flooding generalizes to produce extinction of fear of the shuttlebox light CS. This could reduce the motivation and the reinforcement for responding in the shuttlebox sufficiently to account for the more rapid rate of extinction in the Irrelevant Flood group as compared with the Jump-Up or Home Cage Control groups. By this hypothesis, the effect of the irrelevant flooding treatment should be somewhat smaller than that of the relevant flooding treatment, and this is what was found. Extinction that occurs across CS modalities is not expected to be as complete as extinction of the conditioned response (CR) which has actually undergone the extinction procedure (Konorski, 1948).

If this account or the irrelevant flooding effect is correct, then flooding of the jump-up *avoidance* response should not be necessary to produce the shuttlebox extinction effect. *Any* fear extinction procedure for one CR should also hasten the extinction of the shuttlebox avoidance response, the magnitude of the effect being a function of the similarity of the CSs. Thus, during jump-up flooding, if the critical process that mediates shuttlebox extinction in the Irrelevant Flood group is generalization of Pavlovian fear extinction across CS modalities, then *any* irrelevant Pavlovian fear extinction process (using a CS of a different modality) should also hasten shuttlebox extinction. If, on the other hand, the response made to fear is what changes and generalizes during irrelevant flooding, then an irrelevant Pavlovian fear extinction process should not hasten shuttlebox extinction on the assumption that no specific response to fear had been learned in the conditioning phase that could change and generalize during fear extinction.

EXPERIMENT 2

Experiment 2 was designed to test directly the hypothesis that an irrelevant Pavlovian fear extinction process can hasten the extinction of the shuttlebox response. In order to approximate as closely as possible the amount of fear conditioning in the jump-up box that the Irrelevant Flood group of Experiment I had, the average number of CS–UCS pairings and average duration of UCSs received by that group on each day were used for this experiment. To equate approximately the number of extinction trials, the final 10 consecutive avoidance responses (nonreinforced CSs) on each day for the Irrelevant Flood group were considered to be fear extinction trials, in addition to the 20 flooding trials. The procedure was basically the same as that for the Irrelevant Flood group of Experiment 1, except that instead of receiving avoidance training and flooding in the jump-up box, experimental subjects received fear conditioning and extinction trials there. The control group received fear conditioning trials in the jump-up box but no extinction trials.

Method

Subjects and Apparatus

Subjects were 16 male Fischer rats like those described for Experiment 1. They were housed individually and maintained on ad-lib food and water. The apparatus was identical to that described for Experiment 1.

Procedure

The rats were divided into two groups of eight subjects each. Both groups received shuttlebox avoidance training and fear conditioning in the jump-up box. One group also received fear extinction trials in the jump-up box.

Fear Conditioning and Extinction group ($n=8$). On Day 1, all subjects received shuttlebox acquisition training to a criterion of 10 consecutive avoidance responses. The procedure was identical to that described for Experiment 1. After reaching criterion in the shuttlebox, subjects were placed in the jump-up box on the grid floor (the ledge had been retracted as during flooding). They then received six CS–UCS pairings with a 1-min ITI in the following pattern: (1) 5-sec tone — 1-min shock, (2) 5-sec tone — 20-sec shock, (3) 5-sec tone — 20-sec shock, (4) 5-sec tone — 10-sec shock, (5) 5-sec tone — 5-sec shock, (6) 5-sec tone — 5-sec shock. These shock durations were chosen because they were approximate averages of the pattern of shock durations received by the Irrelevant Flood group of Experiment 1 during their Day 1 acquisition training in the jump-up box. Following these six CS–UCS pairings, the rats received 10 extinction trials, again with a 1-min ITI. The 5-sec tone was presented once a minute 10 times but was no longer followed by shock. These 10 extinction trials were meant to be equivalent to the 10 nonreinforced CSs received by rats of the Irrelevant Flood group during their run of 10 consecutive avoidance responses. The subjects were then returned to their home cages. On Day 2, the subjects were given reacquisition training in the shuttlebox to the same criterion. After reaching criterion they were placed in the jump-up box where they received one CS–UCS pairing: 5-sec tone — 10-sec shock. This was followed by 30 CS extinction trials with a 1-min ITI. Following these fear conditioning and extinction trials they were placed back in the shuttlebox for 40 trials of extinction. The extinction procedure was identical to that described for Experiment 1.

Fear Conditioning Control group ($n = 8$). On Day 1, all rats received acquisition training in the shuttlebox to a criterion of 10 consecutive avoidance responses. The procedure was identical to that described for Experiment 1. After reaching criterion in the shuttlebox, subjects were placed on the grid floor of the jump-up box (with the ledge retracted) where they received six CS–UCS pairings identical to those described for the Fear Conditioning and Extinction group. Upon completion of fear conditioning, subjects were immediately returned to their home cages. They received no CS extinction trials. On Day 2, subjects were placed in the jump-up box for one CS–UCS pairing (5-sec tone — 10-sec shock) like that received by the Fear Conditioning and Extinction group. They were then returned to their home cages immediately for 30 min (instead of being given 30 CS extinction trials) before being replaced in the shuttlebox for 40 extinction trials. These procedural differences are clarified by Table 2, which also presents acquisition and extinction results for this experiment.

TABLE 2

PROCEDURE AND ACQUISITION AND EXTINCTION RATES IN EXPERIMENT 2

Group[a]		Procedure
Fear Conditioning and Extinction		
	Day 1	Shuttlebox acquisition (38.7; 15.5) Light CS
		Jump-up fear conditioning—6 trials Tone CS
		Jump-up CS extinction—10 trials Tone CS
	Day 2	Shuttlebox acquisition (19.4; 4) Light CS
		Jump-up fear conditioning—1 trial Tone CS
		Jump-up CS extinction—30 trials Tone CS
		Shuttlebox extinction (9.4) Light CS
Fear Conditioning Control		
	Day 1	Shuttlebox acquisition (31.5; 11) Light CS
		Jump-up fear conditioning—6 trials Tone CS
	Day 2	Shuttlebox acquisition (14.5; 3) Light CS
		Jump-up fear conditioning—1 trial Tone CS
		30 min in home cage
		Shuttlebox extinction (24.4) Light CS

Note. Numbers in parentheses for acquisition represent the average number of trials to criterion and the average number of shocks received, respectively. Numbers in parentheses for extinction represent the average total number of avoidance responses. CS = conditioned stimulus.

[a] $n = 8$ for both groups.

Results

Acquisition and Reacquisition

As in Experiment 1, minor differences between the two groups did exist on Day 1 in the number of trials to criterion in the shuttlebox and in the number of shocks received there. The Fear Conditioning and Extinction group took more trials to criterion and more shocks than did the Fear Conditioning Control group (both $U\text{s} = 13$, $p < .05$). No differences between the two groups existed on Day 2 acquisition measures. Again, no significant correlations were found within either group between the two acquisition measures and speed of extinction. Therefore, these Day 1 acquisition differences were not considered to be important in interpreting the extinction results.

Extinction

The Pavlovian fear extinction of one CR can have a profound effect on hastening the extinction of a shuttlebox avoidance response learned to a CS of a different modality. The Fear Conditioning and Extinction group of this experiment showed faster extinction of the shuttlebox avoidance response than did the Fear Conditioning Control group ($U = 10$, $p < .02$) Figure 2 presents the mean number of avoidance latency responses for both groups across the four blocks of extinction. In fact the Fear Conditioning and Extinction group made fewer avoidance responses than the Fear Conditioning Control group when the first 20, 30, or 40 trials of extinction are considered together ($U = 13.5$, $p < .058$; $U = 11.5$, $p < .034$; $U = 10$, $p < .02$, respectively).

It is also interesting to compare the results of the groups of this experiment with some of the groups of Experiment 1 which were run under comparable conditions. As expected, the Fear Conditioning and Extinction group of this experiment extinguished considerably faster than the Home Cage Control group (very similar to the Fear Conditioning Control group of this experiment) of Experiment 1 ($U = 16$, $p < .05$). The Fear Conditioning and Extinction group also did *not* differ from the Relevant or Irrelevant

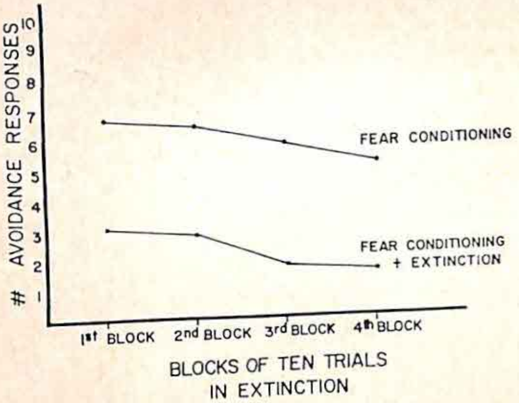

FIGURE 2. Mean number of avoidance latency responses across four blocks of extinction for the Fear Conditioning and Extinction and the Fear Conditioning Control groups of Experiment 2.

Flood groups of Experiment 1 ($U = 33, p > .10$, $U = 37.5, p > .10$, respectively). Thus the fear conditioning and extinction process of the Fear Conditioning and Extinction group was as effective in hastening the extinction of the shuttlebox response as were the relevant or irrelevant flooding treatments of Experiment 1. Fear conditioning without extinction in the jump-up box was clearly not sufficient to produce this effect: The Fear Conditioning Control group did not differ from the Home Cage Control group of Experiment 1 ($U = 37.5, p > .10$).

Discussion

The results of Experiment 2 indicate that the process of Pavlovian extinction of one fear CR can hasten the extinction of the shuttlebox avoidance response which has been learned to a different CS. This finding strongly suggests that the extinction seen in the Irrevelant Flood group of Experiment 1 was also mediated by generalization of fear extinction across CS modalities, especially in light of the strong similarity of procedure and results for the two groups.

On the other hand, these results do not support the idea that irrelevant flooding produces its effect through a response generalization effect for two reasons. First, the response theories of flooding place no importance on the extinction of fear in mediating the effects of flooding; yet an irrelevant fear extinction process appears to do as well as flooding in hastening extinction of a shuttlebox response. Second, the Fear Conditioning and Extinction group was required to learn no specific response to fear in the conditioning phase so it is difficult to envision what alternative or competing response could have been acquired in the extinction phase that could have generalized to the shuttlebox. Thus, at present the most parsimonious explanation of the results of Experiments 1 and 2 seems to be that Pavlovian fear extinction and generalization of extinction across CS modalities underlie the effects of relevant flooding, irrelevant flooding, and irrelevant fear extinction, respectively.

EXPERIMENT 3

Experiment 3 was designed to investigate the question of the generality of the irrelevant flooding results by comparing the effects of relevant and irrelevant flooding on the jump-up response. The strategy is like that of Experiment 1. The effects on extinction of the jump-up response, produced by flooding in the jump-up box (relevant), are compared with those produced by flooding in the shuttlebox (irrelevant).

Method

Subjects and Apparatus

Subjects were 24 male albino rats like those described in Experiment 1. They were housed individually and maintained on ad lib food and water. The apparatus was identical to that described for Experiment 1.

Procedure

In all, three groups of rats were run, with eight rats in each group. As in Experiment 1, the experiment was divided into six phases over the course of 2 days.

Day 1. On Day 1, all three groups received avoidance acquisition training in both the jump-up box and the shuttlebox until they had reached a criterion of 10 consecutive avoidance responses in both situations. The procedure was identical to that described for Experiment 1.

Day 2. Table 3 clarifies the procedural differences for the three groups on Day 2.

TABLE 3

PROCEDURE AND ACQUISITION AND EXTINCTION RATES IN EXPERIMENT 3

Group[a]	Procedure
Relevant Flood	
Day 1	Jump-up acquisition (18.1; 4.7) Tone CS
	Shuttlebox acquisition (37.0; 15.7) Light CS
Day 2	Shuttlebox acquisition (17.4; 6.3) Light CS
	Jump-up acquisition (15.7; 2.4) Tone CS
	Jump-up flood—40 trials Tone CS
	Jump-up extinction (12.9) Tone CS
Irrelevant Flood	
Day 1	Jump-up acquisition (16.2; 3.2) Tone CS
	Shuttlebox acquisition (44.0; 20.6) Light CS
Day 2	Jump-up acquisition (13.9; 1.5) Tone CS
	Shuttlebox acquisition (17.2; 3.2) Light CS
	Shuttlebox flood—60 trials Light CS
	Jump-up extinction (39.0) Tone CS
Home Cage Control	
Day 1	Jump-up acquisition (13.1; 2.9) Tone CS
	Shuttlebox acquisition (34.7; 12.9) Light CS
Day 2	Shuttlebox acquisition (16.9; 5.3) Light CS
	Jump-up acquisition (11.4; .4) Tone CS
	40 min in home cage
	Jump-up extinction (37.5) Tone CS

Note. Numbers in parentheses for acquisition represent the average number of trials to criterion and the average number of shocks received, respectively. Numbers in parentheses for extinction represent the average number of avoidance responses. CS = conditioned stimulus.

[a] $n = 8$ for all groups.

Relevant Flood group ($n = 8$). Each rat was first given reacquisition training in the shuttlebox, using the same procedure as that used on Day 1. Immediately after reaching criterion, rats were placed in the jump-up box where they received reacquisition training to the same criterion. Following the 10th avoidance response, the ledge was manually disconnected by the experimenter and the shock was turned off. After the last ITI, the rat was dropped onto the grid but not allowed to jump back on the ledge which remained retracted. This was the beginning of a 40-min flooding session during which no jump-up responses were allowed. The 5-sec CS (tone) was presented once a minute (40 times). At the end of the 40-min flooding session, the rat was removed from the apparatus for 1 min to allow the experimenter to reconnect and reinsert the ledge. Extinction began by re-placing the animal on the ledge, thus starting a new ITI. Fifty seconds later the animal was dropped on the grid floor but no more shocks were presented. Extinction continued until the animal had made a total of 40 responses (escape or avoidance latency) or had failed to respond at all for at least 10 min.

Irrelevant Flood group ($n = 8$). Subjects were placed in the jump-up box where they received reacquisition training to the usual criterion of 10 consecutive avoidance responses. They were then placed in the shuttlebox until they reached the same criterion number of avoidance responses. When the subjects had reached this criterion the metal barrier described in Experiment 1 was inserted by the experimenter and the shock was disconnected. A series of 60 flooding trials then began. As described in Experiment 1, CSs were presented on the usual VT 1-min schedule, but no responses were allowed. Following this 1-hr flooding session in the shuttlebox, subjects were placed back on the ledge in the jump-up box for the start of extinction. As for the Relevant Flood group, extinction lasted until the subject had made a total of 40 responses or had failed to respond at all for at least 10 min.

Home Cage Control group ($n = 8$). This group received exactly the same treatment as the Relevant Flood group, except that instead of receiving 40 flooding trials in the jump-up box after reaching criterion there, the Home Cage Control group spent this 40 min in their home cages. Then they were placed back in the jump-up box for the usual extinction procedure described above.

Results

Acquisition and Reacquisition

Table 3 presents the major results of this experiment. As in Experiment 1, some minor differences existed between groups in trials to criterion and in number of shocks received in the jump-up box. On Day 1, rats in the Relevant Flood group took more trials to criterion and received more shocks in the jump-up box than did rats in the Home Cage Control group ($U = 8.5$, $p < .014$, and $U = 10.5$, $p < .028$, respectively). Similarly, on Day 2 rats in the Relevant Flood group took more trials to reach criterion and received more shocks in the jump-up box than did those in the Home Cage Control group ($U = 8.5$, $p < .014$, and $U = 6$, $p < .004$, respectively). In addition, rats in the Irrelevant Flood group took more shocks than those in the Home Cage Control group in the jump-up box on Day 2 ($U = 12.5, p < .05$). There were no significant differences between groups on acquisition measures in the shuttlebox.

As in Experiment 1, these acquisition differences were not judged to be important in interpreting the extinction differences between groups: No significant correlations existed for any of the three groups between either of the acquisition measures on either day and number of avoidance responses in extinction (all r_ss *ns*, Spearman rank-order correlation).

Extinction

Only the relevant flooding treatment enhanced the rate of extinction of the jump-up response. Irrelevant flooding had no effect on this response. Kruskal-Wallis analysis of variance revealed a significant difference between the groups in number of avoidance responses in extinction ($H = 10.94, p < .01$). Figure 3 shows the mean number of avoidance responses for each group across the four blocks of extinction trials. Specifically, flooding in the jump-up box markedly reduced resistance to extinction for the Relevant Flood group. Four out of eight animals made no responses in extinction following flooding. In contrast, 40 min in the home cage or 60 min of flooding in the shuttlebox resulted in no extinction whatsoever in the jump-up box for any subject. The Relevant Flood group made fewer avoidance responses than the Irrelevant Flood group ($U = 7, p < .006$) and the Home Cage Control group ($U = 6, p < .004$), which did not differ from each other. Another way of looking at these results is to look at the number of subjects in each group making more than 30 out of 40 possible avoidance responses in extinction. Whereas 88% and 100% of the Home Cage Control and Irrelevant Flood groups made at least 30 out of 40 avoidance responses, only 25% of the Relevant Flood group met this criterion. This difference was significant on a χ^2 test ($\chi^2 = 12.53, p < .01$).

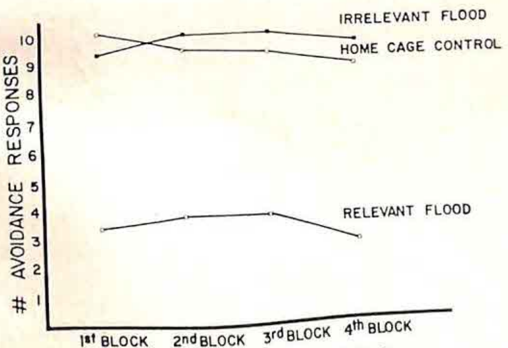

FIGURE 3. Mean number of avoidance latency responses across four blocks of extinction for Relevant Flood, Irrelevant Flood, and Home Cage Control groups of Experiment 3.

Discussion

The results of Experiment 3 indicate that the effects of irrelevant flooding may not be as general as are the effects of relevant flooding. In this experiment, 60 trials of flooding in the shuttlebox produced no apparent effect on the extinction of the jump-up response. Sixty flooding trials were used to maximize the possibility of hastening extinction of the jump-up response.

There seem to be two possible explanations for the asymmetry of the irrelevant flooding results seen in Experiments 1 and 3. First, it is possible that irrelevant flooding hastens only shuttlebox extinction because *different* learning processes may be involved in the flooding of different avoidance responses. This suggestion was first made with regard to relevant flooding by Baum (1970), who concluded that the account of flooding that is most correct may depend on the particular avoidance response being studied, as well as the particular parameters of acquisition, shock intensity, etc. By this hypothesis, shuttlebox flooding may involve only fear extinction, but jump-up flooding may involve both some fear extinction and the learning of a new response. Thus irrelevant flooding may hasten shuttlebox extinction because of generalization of fear extinction across CS modalities. Irrelevant flooding may not, however, hasten jump-up extinction because, although fear extinction may generalize, no new response may have been learned in the shuttlebox to generalize to the jump-up box.

There is a second possible alternative explanation for the asymmetry of the irrelevant flooding results. The chief effect of flooding may be to allow for fear extinction to occur both in the shuttlebox and in the jump-up box. However, because the stimulus eliciting the fear may be quite different in the two different situations, flooding may be reducing the fear of two quite different stimuli. A number of investigators have pointed to the very different nature of two-way shuttlebox learning and one-way (jump-up box) learning (e.g., Bolles, 1970; Seligman, Maier, & Solomon, 1971; Turner & Solomon, 1962). Fear of the actual CS seems to be the important controlling factor in two-way shuttlebox responding, at least early in acquisition, but fear of the CS may be much less important in one-way responding (Bolles, 1970). One-way (jump-up) avoidance learning can probably best be conceived of as place learning with most of the fear being conditioned to the dangerous side of the box rather than to any nominal CS. So flooding in the shuttlebox may reduce fear of the CS but flooding in the jump-up box may primarily reduce fear of the dangerous part of the box (i.e., the floor) with some incidental reduction in fear of the CS also occurring. By this analysis, then, we might predict the asymmetry of the irrelevant flooding effect. Fear extinction to the CS that occurs in the jump-up box may generalize to produce fear extinction of the CS in the shuttlebox, and hence hasten extinction of the shuttlebox avoidance response (Experiment 1). On the other hand, fear extinction to the CS that occurs in the shuttlebox may not generalize to produce fear extinction of the floor in the jump-up box, and because fear of the floor controls jump-up responding, the response does not extinguish (Experiment 3). One would expect the generalization of fear extinction to be a function of the similarity of the CSs (Pavlov, 1927), and certainly a 5-sec tone and a 5-sec light CS are more similar than are a 5-sec light and a grid floor.

A decision as to which of these two alternative explanations of the asymmetry of the irrelevant flooding effect is correct awaits further investigation.

General Discussion

The three experiments reported here raise some interesting questions about the specificity of the effects of flooding and about the possibility that different learning processes may be involved in the flooding of different kinds of avoidance responses. Experiments 1 and 2 clearly indicate that flooding can have transsituational effects and that such effects are probably mediated by generalization of fear extinction. Experiment 3 indicates that what the transsituational effects of flooding are may depend on the learning process occurring during flooding a particular response or on the effective stimulus controlling that response.

One note of caution in interpreting the results of these experiments should be observed. Baum and LeClerc (1974) demonstrated that 5 min of a stressful situation (swimming) produced as large a decrement in jump-up avoidance responding as did 5 min of flooding. However, when 2 hr were interspersed between the swimming or flooding treatment and extinction, the swimming group showed no response decrement compared with an untreated control group. The flooded group did show faster extinction even after this 2-hr period. Because stress effects can mediate extinction of the jump-up response only if extinction occurs immediately, the authors concluded that such irrelevant stress is mediating extinction of the response through a different process than that occurring during flooding of the response. These time-course variables may also prove to be important in the irrelevant and fear extinction treatments of the experiments reported here. It is possible that the shuttlebox extinction effects in the Irrelevant Flood and the Fear Conditioning and Extinction groups might not occur if a considerable period of time elapsed between the treatment and extinction of the shuttlebox response. This might not be too surprising if the early results on generalization of extinction (e.g., Konorski, 1948) are correct, for these early results suggested a decrease in the secondary extinction effect if more than 5–10 min elapsed following primary extinction. Certainly such a comparison across the Relevant

Flood, the Irrelevant Flood, and the Fear Conditioning and Extinction groups is necessary before any strong conclusions can be made about the generality of the irrelevant flooding and fear extinction treatments.

Nevertheless, the results of Experiments 1 and 2 provide an interesting parallel to those reported by Watson and Marks (1971) on the effects of irrelevant flooding of human phobias. The fact that flooding can have transsituational effects, possibly mediated by generalization of fear extinction, raises intriguing questions about the mechanisms underlying flooding of different avoidance responses and of human phobias.

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Observing as a Function of Stimulus Difference

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The relative effectiveness of alternative pairs of discriminative stimuli as reinforcers of observing was compared by providing them to pigeons on concurrent variable-interval schedules as consequences of pecking different keys. In Experiment 1, the pigeons pecked at a higher rate on a key that produced a large difference in the levels of illumination used for positive and negative stimuli than they did on a key that produced a small difference. This finding increases the plausibility of attentional interpretations of a variety of phenomena in the area of stimulus control. In Experiment 2, we divided the same continuum into two parts. As the magnitude of the difference between the positive stimulus and the mixed-schedule stimulus increased, the rate of pecking the observing key likewise increased. But as the magnitude of the difference between the negative stimulus and the mixed-schedule stimulus increased, the rate decreased. These results suggest that differences between discriminative and background stimuli may be critical, rather than the difference between the two discriminative stimuli.

The first of our two experiments was prompted by reports in the literature suggesting that animal subjects pay more attention to discriminative stimuli that are well separated along some physical dimension than to comparable stimuli that lie closer together. In other words, they attend better to large differences than to small. This relationship seemed to be a particularly important one to investigate because it apparently enters into a number of different situations that are of interest in the area of stimulus control.

For example, one of the phenomena in which attention is thought to play a significant role (e.g., Mackintosh and Little, 1970; Sweller, 1972) is the easy-to-hard transfer effect. As was originally noted by Pavlov (1927/1960, pp. 121–123), an animal learns to discriminate between two very similar stimuli (hard discrimination) more rapidly or more effectively if the first part of his training is devoted to less similar stimuli (easy discrimination) from the same dimension. An attentional analysis of this phenomenon suggests that while being trained on the easy discrimination the animal learns relatively quickly to attend to the relevant dimension; subsequently, when transferred to the difficult discrimination, the animal continues to attend to the appropriate stimuli to a greater degree than if it had been trained from the beginning on the difficult discrimination.

Another phenomenon that has been a candidate for explanation in terms of attention (e.g., Sutherland and Mackintosh, 1971) is that of overshadowing. This, too, was first noted by Pavlov (1927/1960, pp. 141–143). In a demonstration of overshadowing that is particularly relevant to the present experiments, Miles and Jenkins (1973) provided their pigeons with compound discriminative stimuli that included cues from two different dimensions. The positive stimulus consisted of illumination of the key combined with the sounding of a tone; the negative stimulus was a lower level of illumination and the ab-

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sence of the tone. The specific level of illumination used for the negative stimulus varied with the different groups. Miles and Jenkins found that the degree of control exerted by the light was a positive function and the degree of control exerted by the tone a negative function of the magnitude of the difference in illumination. In other words, as the light dimension became more effective, it increasingly overshadowed the tone. An interpretation of this relationship in terms of attention would suggest that discriminative stimuli differing by a large amount acquire a greater share of the subject's attention than do stimuli differing by a small amount.

The same relationship between magnitude of difference and degree of control is also found in certain experiments on fading. In a study by Westbrook and Miles (1970), for example, a tone that was correlated with the reinforcement of key pecking was initially overshadowed by the light that accompanied it. When a light of lower intensity was added to the procedure, however, as part of the negative stimulus compound, and was gradually raised to higher levels, control of key pecking shifted to the tone. In this case, the assumption is that the subject's attention shifted from the light-intensity dimension to the tone as the magnitude of the difference in illumination was reduced.

A similar relationship has been reported by Feldman (1975). Initially, a tone served by itself as the positive stimulus for bar pressing by rats. Later, a light was added, in compound with the tone. Control by the light was reduced by the prior training with the tone—an effect known as blocking—but the greater the intensity of the light the less it was blocked by the tone.

As we have noted in the individual cases, the assumption required in common for an attentional interpretation of each of these findings is that the subject learns to attend more quickly or more fully to discriminative stimuli that differ by a substantial amount than to stimuli that lie close together along the continuum. The early models formulated by Zeaman and House (1963) and by Lovejoy (1968) cannot handle most of the experiments we have cited, since these models pertain only to situations in which the subject can increase the frequency of reinforcement by performing the correct response. In its final form, the basic rule employed by Sutherland and Mackintosh (1971, p. 64) for strengthening an "analyzer" (their representation of attention) was very much like the rule initially proposed by Wyckoff (1952) for the strengthening of an observing response. Both rules were based on the magnitude of the difference in the strength of the response to the positive and the negative stimulus. Wyckoff focused on the production of one of the stimuli as the reinforcing event and hypothesized that the larger the difference in the instrumental response at the time, the more effective the stimulus was in strengthening the observing response. Sutherland and Mackintosh focused on the reinforcing or nonreinforcing outcome of the response to a given discriminative stimulus on a given trial. The more this outcome increased the difference in response strength to the two stimuli (formally, to the outputs from the analyzer), the more it strengthened the relevant analyzer. Given this rule, generalization between the two stimuli has an important effect (Sutherland & Mackintosh, 1971, pp. 47 & 65). When the stimuli lie close together, one stimulus is sometimes mistaken for the other, and the difference in response strength may decrease, rather than increase, on a given trial. Therefore, on the average, the relevant analyzer should be strengthened relatively slowly and should approach a lower asymptote. When the stimuli lie farther apart, such mistakes are rare; the relevant analyzer will be strengthened more rapidly and will approach a higher asymptote.

Whether greater attention is, in fact, directed toward discriminative stimuli as a function of such factors as the magnitude of the difference between them or the magnitude of the difference in responding to them is difficult to say. Attention is a hypothetical construct. As such, it is not observable, and the only evidence thus far offered for the proposition outlined above has been data of the very sort it was summoned to explain. The weakness of such reasoning is revealed by the fact that an alternative model (Rescorla and Wagner, 1972) has been able to account for many of the same data with substantially fewer assumptions, none of which make any mention of the concept of attention.

As Sutherland and Mackintosh have pointed out (p. 40), it is not necessary to assume that analyzers and responses are strengthened according to the same rules. On the other hand, responses are observable, and therefore their relationship to various experimental parameters can be determined with a minimum of ambiguity. We maintain that a relationship that can be shown to hold for at least some item of behavior has an advantage over a relationship that remains entirely hypothetical. If the empirically observed relationship is equally consistent with the outcomes of experiments on attention, for example, it is to be preferred to relationships that are evidenced solely by their consistency with these same outcomes.

To test the hypothesis that the effectiveness of discriminative stimuli as reinforcers is a function of the magnitude of the difference between them we used a concurrent observing procedure. The discriminative stimuli produced by pecking one of the two observing keys differed from each other by a certain amount; the stimuli produced by pecking the other key differed, on a given block of sessions, by a smaller or by a larger amount. The relative rate of pecking on the two keys served as a measure of the relative effectiveness of each pair of stimuli as conditioned reinforcers.

EXPERIMENT 1

Method

Subjects. Eight female White Carneaux pigeons, about 6 years old, served as subjects. Following each experimental session, the bird was provided with sufficient grain to maintain a body weight approximately 75% of what it had been under ad libitum feeding. Two groups served in successive replications of each of the two experiments, under slightly different procedures. In the first group, Bird 2151 yielded such a low rate of pecking on the food key that we replaced it with Bird 1113. Because of the late start, we collected data from 1113 only during the second experiment. Birds 2502 and 6622 completed both experiments. In the second group, Bird 900 died following the first experiment, but Birds 1005, 2371, and 3921 served in both experiments.

Apparatus. The birds were tested in a 3-key Lehigh Valley pigeon chamber, Model 1519. The panel bearing the keys and the feeder was 35.2 cm wide and 34.9 cm high, measuring from the mesh floor. From this panel the experimental space extended 30.8 cm to the rear wall. Three response keys, made of a light-diffusing plastic, were mounted behind circular openings 2.5 cm in diameter. These openings were arranged in a horizontal row 25.4 cm above the floor. The left and right hand keys were centered about 11.4 cm from the respective side walls and 6.2 cm from the middle key. The force required to register a response ranged from 0.19 to 0.22 N on the side keys and from 0.11 to 0.14 N on the center key. Each key was illuminated from the rear by a Type 757 bulb; the original supply for these bulbs was 28 V DC but could be reduced to any one of seven values, depending on which of seven variable resistors was placed in series by the switching circuitry. The bulbs operated in parallel, and varied in brightness together. Changes in brightness were accompanied by changes in color temperature, which were roughly although not linearly proportional in magnitude. For convenience of exposition only the brightness will be mentioned in subsequent discussion, since the nature of the physical dimension is immaterial to the purposes of the present experiments. Each stimulus will be specified in terms of the voltage delivered to the bulbs.

The opening to the food hopper was 5.1 cm high and 5.7 cm wide. It was centered laterally, with its lower edge 9.8 cm above the floor. The hopper itself made a highly audible thump when raised to an accessible position, and was lighted by a Type 757 (.08 A) bulb, operated at 28 V DC.

Experimental events were scheduled by a circuit utilizing solid-state switching modules manufactured by the Massey Dickinson Co., and the data were collected on electromagnetic counters, running time meters, and Gerbrands Type SHS cumulative recorders. The control and recording circuitry was housed in other rooms, and a blower attached to the experimental chamber provided a masking noise, in addition to circulating the air.

Procedure. Since an earlier attempt had yielded inconclusive results, for the present study we adopted a measure that is believed to be relatively sensitive to magnitude of reinforcement (e.g., Catania, 1963; Herrnstein, 1970). That is, we used the relative rate of responding on two keys under concurrent variable-interval (VI) schedules. In our case, pecking the left-hand observing key produced one set of discriminative stimuli and pecking the right-hand observing key produced another.

The pattern of performance necessary for such a comparison was established in four successive stages. Experimental sessions were scheduled for 90-min periods at the same time each day. First, we covered the side keys with opaque strips of plastic tape and trained the birds to peck the center key, which was illuminated at a level of 23 V, by reinforcing successive approximations to the final pattern. The reinforcement was 2.5 sec of access to a hopper containing mixed grain, which was raised and lighted at appropriate times. After a brief period in which all pecks were reinforced, a VI schedule was instituted. Under this schedule, the first peck in each successive interval of time was reinforced, and the length of the intervals varied unpredictably. Initially, the mean interval between reinforcements was 8 or 10 sec, but during the next several (five to eight) sessions it was gradually extended to 120 sec.

The next stage was discrimination training. For the first group of birds (1113, 2151, 2502, and 6622), a brighter illumination of the key (28 V) was the positive stimulus and a dimmer illumination (18 V) the negative. For the second group (900, 1005, 2371, and 3921) a dim illumination (19 V) was the positive stimulus and a bright illumination (27 V) the negative. During this training the positive and negative stimuli alternated at intervals determined by presenting an electric signal every 10 sec to a probability gate set at one in five. Thus, the mean duration of each stimulus was 50 sec. When the positive stimulus was present, pecking the key was reinforced on a VI 60-sec schedule. The same schedule was used throughout the remainder of the experiment. Ten intervals were employed, ranging from 7 to 175.5 sec; the specific intervals were selected on the basis of a formula for a "constant probability VI" suggested by Catania and Reynolds (1968, p. 381). In the presence of the negative stimulus, no food was delivered.

After six sessions of discrimination, the birds were trained to peck the side keys, which were illuminated at the intermediate or neutral level of 23 V. First, the center key was covered with plastic tape and the left key was uncovered. Pecking was reinforced for one session on a VI 60-sec schedule. Then the left key was covered again, the right key was uncovered, and pecking this key was reinforced for one session on a VI 60-sec schedule.

Throughout the remainder of the experiment the following conditions held. All three keys were available to the bird, and all three were illuminated at the same level. Normally, that is, prior to the production of a discriminative stimulus, the potential applied to the lamps behind the keys was 23 V regardless of which schedule was in effect for the delivery of food; hence, 23 V could be considered a mixed-schedule stimulus. To turn on the discriminative stimuli, it was necessary that the bird peck one of the observing (left or right) keys. On equal but independent VI 30-sec schedules, pecking a given side key produced a 30-sec display of the discriminative stimuli. Times when the stimuli were displayed were not included in either schedule. During the 30 sec of the display, the VI schedule on the food (center) key was accompanied by the positive level of illumination on all keys and the extinction schedule by the negative level. If the schedule of reinforcement shifted from VI to extinction or from extinction to VI during this 30-sec period, the level of illumination shifted with it.

To reduce the possibility that adventitious contingencies might distort the data, two changeover delays were included in the circuit. First, no grain was ever received within 3 sec after a response on either of the observing keys. If originally scheduled for such a period, it was delivered following the first peck on the food key after the period had elapsed. Second, the food key schedule and its associated stimuli could not shift from extinction to VI within 5 sec after a peck on the food key.

The independent variable in this experiment was the difference in level of illumination (voltage) between the positive and the negative stimulus produced by pecking a given observing key. With the first group of birds, we began manipulation of this factor only after they had been on the observing procedure for 42–45 sessions. The magnitude of the change produced by pecking each of the keys is listed for successive blocks of sessions in Table 1. Each block lasted for 12 sessions.

The second group of birds was trained for a number of sessions with 19 V as the positive stimulus and 27 V as the negative. That is, the change in intensity was 4 V, producing a total difference of 8 V regardless of which key had been pecked. On session 36, however, the change produced by pecking the left key was reduced to 2 V in either direction. When the rate of responding on this key dropped to a low level, from which it might not readily recover, the conditions were reversed, so that pecking the left key produced a change of 4 V but pecking the right key produced a change of 2 V. Reversal was continued as needed but in any case occurred after not more than 15 sessions, so that systematic data could be collected while the training continued. Fifteen sessions with a change of 2 V produced on the left key and only 1 V on the right were included as the next to last block before proceeding to Experiment 2.

Results

The nature of the day-to-day data from which our final summaries were constructed is illustrated in Figure 1. The absolute rate of pecking on each of the two observing keys is plotted for Bird 900 for each successive session throughout the period of testing. Times when the positive or the negative stimulus was present, however, have been excluded from the calculation; hence, the rates refer to the bird's performance in the presence of the neutral or mixed-schedule stimulus (23 V). The first number in each panel indicates the magnitude (in volts) of the change produced by pecking the left ob-

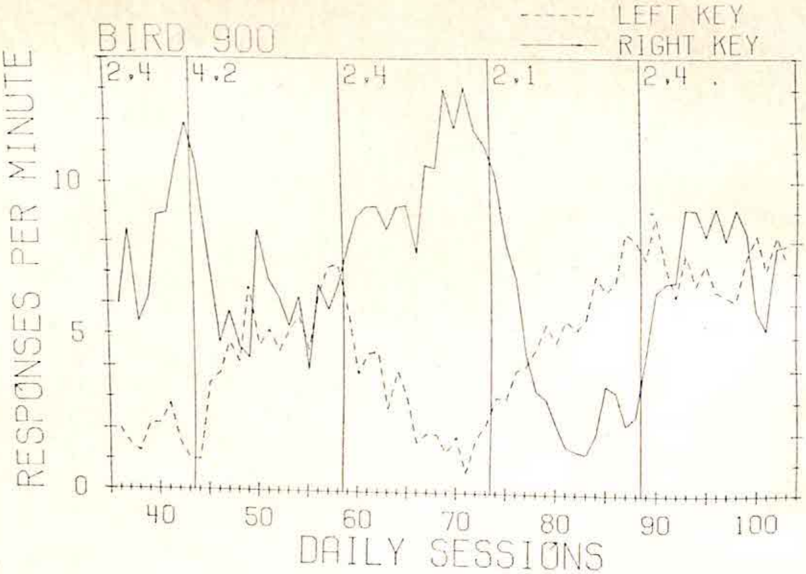

FIGURE 1. Rate at which Bird 900 pecked each of the observing keys during successive sessions. (The first number at the top of each panel designates the magnitude of the change in illumination, measured in volts, produced by pecking the left key; the second number is that produced by pecking the right key. The higher and lower levels of illumination served as discriminative stimuli for pecking the center, or food, key.)

serving key, the second number the change produced by pecking the right observing key. Although there are some irregularities in the data, it is clear that the rate is sensitive to these parameters. On most sessions, the rate is higher for the key producing the larger change, and the change from one block to the next is always in the appropriate direction.

Since the sequence of stimulus values was different for the two groups, the summary data for Birds 2502 and 6622 (higher illumination as positive stimulus, lower as negative) are presented in Table 1 and the data for Birds 900, 1005, 2371, and 3921 (lower illumination as positive stimulus, higher as negative) are presented in Table 2. The performance on a given session was assessed by dividing the number of pecks on the left key by the total number of pecks on both keys; medians were then obtained for the last 5 sessions in each block and entered in the table. Since the period of time for which pecking was recorded was the same for either key, these entries can be treated either as proportions or as relative rates. In all cases but two, the bird shows a preference for the key that produces the larger change in illumination, and in all cases the change in preference from one block of sessions to the next is in the appropriate direction.

Discussion

In our first experiment, then, the relative rate of responding on the two observing keys was a positive function of the relative size of the difference between the two stimuli that each key produced. Further data along these lines may be found in Table 3 of Experiment 2. Taken together, these data indicate that discriminative stimuli are more effective reinforcers when the difference between them is relatively large than when it is extremely small. Such a conclusion increases the plausibility of attentional interpretations of a nu ber of experiments, like those cited in c introduction, in which separation along t physical continuum was a parameter of stin ulus control, since it points to a mechanisn by means of which greater observing and perhaps also greater attention may be directed toward stimuli that differ by a large amount. Presumably the subject observes, and perhaps also attends to these stimuli, more quickly and more fully, possibly to the detriment of other stimuli which bear the same relationship to the schedule of reinforcement.

Despite the theoretical importance of this relationship, however, consideration of previous data on the reinforcement of observing suggests that our analysis is not yet complete. When the positive and the negative members of a given pair of discriminative stimuli have been tested independently, the positive stimulus appears to have been the sole source of reinforcement (e.g., Browne and Dinsmoor, 1974; Dinsmoor, Browne, and Lawrence, 1972). The negative stimulus exercises a suppressive effect (Jwaideh and Mulvaney, in press; Mulvaney, Dinsmoor, Jwaideh, and Hughes, 1974.) Therefore, it seemed as though it might be instructive to divide the total range between the positive and the negative stimulus into two parts and to assess the effect of each part separately. The relationship found in Experiment 1 between magnitude of stimulus change and rate of observing may have been due wholly to the magnitude of the change to the positive stimulus. The function relating rate of observing to the magnitude of the difference between the mixed-schedule stimulus and the negative stimulus may be quite different. Accordingly, in Experiment 2 we separated the two stimulus values and varied them orthogonally. Four levels of the negative stimulus—including no change, as the end point of the continuum—were tested in combination with two levels of the positive stimulus, in a factorial design.

TABLE 1

PROPORTION OF PECKS OCCURRING ON LEFT KEY AS A FUNCTION OF MAGNITUDE OF CHANGE IN BRIGHTNESS (VOLTAGE) PRODUCED BY PECKING LEFT OR RIGHT OBSERVING KEY, EXPERIMENT 1

nitude of Change		Median proportions[a]	
t Key	Right Key	Bird 2502	Bird 6622
4	2	.892	.901
1	2	.025	.108
4	2	.732	.741
1	2	.020	.359
2	4	.252	.377

[a] Median proportions were obtained for last five sessions in each block.

TABLE 2

PROPORTION OF PECKS OCCURRING ON LEFT KEY AS A FUNCTION OF MAGNITUDE OF CHANGE IN BRIGHTNESS (VOLTAGE) PRODUCED BY PECKING LEFT OR RIGHT OBSERVING KEY, EXPERIMENT 1

Magnitude of Change		Median proportions[a]			
Left Key	Right Key	Bird 900	Bird 1005	Bird 2371	Bird 3921
4	2	.525	.763	.804	—
2	4	.133	.355	.310	.217
2	1	.760	1.000	.699	.998
2	4	.512	.673	.241	.429

[a] Median proportions were obtained for last five sessions in each block.

EXPERIMENT 2

Method

Procedure. The training the birds received in Experiment 1 provided an appropriate preparation for Experiment 2. The daily routine remained the same in all respects, save for the combinations of positive and negative stimuli that were employed. The consequences of pecking what we will call the constant key (left for Birds 1005, 1113, 2502, and 6622; right for Birds 2371 and 3921) remained the same throughout all the experimental sessions: a change of 2 V in one direction to produce the positive level of key illumination and a change of 2 V in the opposite direction to produce the negative level. The rate of responding on this key provided a basis of comparison for the rate of responding on the other key, which we will call the variable key. Eight different combinations of positive and negative stimuli were tested in successive blocks of sessions as consequences of pecking the variable key. Two levels of key illumination (changes of 2 V and 4 V) were used for the positive stimulus, and four levels of key illumination (changes of 0, 1, 2, and 4 V) were tested as negative stimuli at each of these positive values.

The first group of birds, which received a higher voltage for the positive stimulus and a lower voltage for the negative stimulus, was tested for 12 sessions at each combination of values. Since we expected the changes in rate of observing as a function of changes in the negative stimulus to be relatively small, we completed the testing of all four negative stimuli at a given value before shifting the positive stimulus. This minimized the possible disruption of the negative stimulus function by long-term drifts in performance.

The second group of birds, which received a lower voltage for the positive stimulus and a higher voltage for the negative stimulus, was tested for 15 sessions under each combination of values, in order to provide more time for the transition from one level of performance to another. For these birds, we used an ascending or a descending series of negative stimulus values. No two birds, however, received the same overall sequence.

Results

To measure the effect of each combination of positive and negative stimuli on the rate of observing, we calculated the proportion of observing responses that occurred on the variable key. That is, the number of pecks on the key that produced the consequences that varied from one block of sessions to the next was divided by the total number of pecks on both observing keys. Thus, a high value means that the combination of stimuli produced a high rate of pecking, relatively speaking, and a low value signifies a low rate. Only those pecks that occurred in the presence of the mixed-schedule stimulus (23 V) were counted; any pecks that occurred during the presentation of a discriminative stimulus were excluded from the record. The determinations presented in Table 3 are medians for the last 5 sessions within each block.

The data presented in Table 3 indicate that a positive discriminative stimulus that is 4 V above or below the neutral point (23 V) maintains a higher rate of pecking on an observing key than one that differs by only 2 V. This result is highly reliable: Among the 24 pairs of entries to be compared in Table 3 there is only one inversion. A second relationship is that between the negative discriminative stimulus and the rate of pecking on the observing key. The rate is highest when there is no difference in brightness between the negative stimulus and the neutral point and suffers a progressive decline as the magnitude of the difference increases to 1, 2, or 4 V. There are occasional inversions in this function for the individual birds, but the slopes of straight lines fitted by the method of least squares are of appropriate mathematical sign (positive or negative) in 11 cases out of 12. The slope of the function for the negative stimulus does not appear to be as steep, however, as the corresponding slope for the positive stimulus. For example, there is typically a greater difference between entries for 2 and 4 V for the positive stimulus than there is between 0 and 4 V for the negative.

Discussion

In the first experiment, we studied the effects of varying the total difference between the positive and the negative discriminative stimulus. As expected, we found that the rate

TABLE 3

RELATIVE RATES OF PECKING[a] TO PRODUCE TWO LEVELS OF THE POSITIVE DISCRIMINATIVE STIMULUS, MEASURED BY CHANGE IN VOLTAGE, IN COMBINATION WITH FOUR LEVELS OF THE NEGATIVE STIMULUS, EXPERIMENT 2

Negative change	Positive change							
	2	4	2	4	2	4	2	4
	Bird 1113		Bird 2502		Bird 6622		*M*	
0	.706	.844	.656	.901	.672	.715	.678	.820
1	.549	.871	.627	.813	.642	.681	.606	.788
2	.503	.824	.670	.807	.649	.632	.607	.754
4	.635	.731	.518	.748	.510	.623	.554	.701
Slope	−.011	−.032	−.033	−.034	−.040	−.023		
	Bird 1005		Bird 2371		Bird 3921		*M*	
0	.869	.996	.526	.699	.300	.617	.565	.771
1	.671	.676	.447	.618	.329	.567	.482	.620
2	.486	.553	.384	.563	.293	.496	.388	.537
4	.013	.465	.452	.504	.404	.559	.290	.509
Slope	−.214	−.199	−.016	−.047	+.024	−.014		

[a] Median of last five sessions under that condition.

of observing increased as a function of the magnitude of this difference. In the second experiment, we divided the original continuum into two separate parts, one running from the neutral or mixed-schedule stimulus (*23* V) to the positive stimulus, the other running from the neutral stimulus to the negative stimulus. The results of varying each of these sectors were opposite in direction. When it was the magnitude of the difference between the positive stimulus and the neutral stimulus that was varied, the results were as before: The greater the difference, the higher the rate of observing. It was presumably this part of the range that accounted for the results obtained in the first experiment. When it was the magnitude of difference between the negative stimulus and the neutral stimulus that was varied, however, the trend was reversed: The greater the difference, the lower the rate of observing.

While our results support the hypothesis that large differences between discriminative stimuli maintain more observing than small ones, they also raise a question concerning the ultimate basis of this relationship. They suggest that conventional theorizing in this area may be focussing on the wrong dimension. The neutral or mixed-schedule stimulus in our experiment corresponds to the stimuli with which the subject is in contact in stimulus control experiments when it is not observing the current discriminative stimulus. Our results therefore suggest that it is the magnitude of the difference between this background stimulation and each of the discriminative stimuli (salience) that determines the amount of observing rather than the magnitude of the difference between the two discriminative stimuli themselves (disparity). Observing is a positive function of the salience of the positive stimulus but a negative function of the salience of the negative stimulus. We like to think that this finding may apply not only to situations in which contact is made with the relevant stimuli by appropriate orientation of the receptors (natural observing) but also to situations in which it depends on more central processes (attention). If so, it would be desirable in future work in this area to separate the factors of stimulus disparity and stimulus salience more carefully than has typically been the case. The two dimensions are readily confounded. Many of the results which in the past have been attributed to differences in disparity may in reality be attributable to differences in the salience of one or the other stimulus.

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Latent Inhibition and Conditioned Attention Theory

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Four experiments are reported of relevance to a conditioned attention theory of latent inhibition (LI) phenomenon. The conditioning suppositions of the theory predict that the addition of a second stimulus in a conditioning relationship to the preexposed stimulus should maintain attention to that stimulus and thus attenuate LI. Experiments 1 and 2 demonstrated this effect in the context of lick-suppression conditioning. It is further supposed that subject control of the presentation of the preexposed stimulus should maintain attention to that stimulus and thereby reduce LI. This was demonstrated in Experiments 3 and 4, also in lick-suppression conditioning.

Lubow (1973), in reviewing the latent inhibition (LI) literature, concluded that available explanations of the decremental effects of preexposure of the to-be-conditioned stimulus upon subsequent learning were inadequate. The explanations surveyed included habituation of the orienting response (e.g. Wolf & Maltzman, 1968), selective filter biasing (e.g. Schnur, 1971), competing response (Lubow & Moore, 1959), complementing response (Lubow, Markman, & Allen, 1968), conditioned inhibition (Reiss & Wagner, 1972; Rescorla, 1971), and stimulus salience reduction (Reiss & Wagner, 1972; Rescorla, 1971).

More recently, Lubow, Alek, and Arzy (1975) have prepared a conditioned attention theory (here abbreviated CAT) of latent inhibition. The conditioned attention theory is based on the use of attention as a hypothetical construct with the characteristics of a Pavlovian response. The attentional response (R_A) is assumed to be conditionable but to differ from a CR in the typical Pavlovian paradigm in that the CS is assumed to elicit R_A on first presentation. In a Pavlovian conditioning procedure, the CS typically does not initially elicit the response which is to be conditioned but comes to do so after training. CAT assumes that the attentional response to a stimulus is elicited on first presentation, and the function of a consequent US is to *prevent* that R_A from diminishing. It is the *maintenance* of the R_A that is accomplished by the usual conditioning procedures. Therefore, when R_A is maintained over repeated trials with one stimulus, as a result of that stimulus being paired with a second stimulus, R_A should be considered as a conditioned attentional response, CR_A.

While one can speak of the maintenance of an attentional response to a stimulus as being a conditioned response, the fact that the absence of a significant event (such as US) following the stimulus leads to a relatively permanent decrement in associability of that stimulus also suggests a learning process, which may be conceived, in parallel fashion, as the conditioning of inattention to that stimulus. MacKintosh (1975) has recently made a similar proposal to account for the effects of experience on reduced associability using the phrase "learning to ignore," while Wagner (in press) has offered a related treatment using habituation terminology.

Impetus for a conditioning-like approach to LI arises from data that indicate that, without the presentation of a second stimulus

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(in a conditioning relationship to the first stimulus), the amount of latent inhibition increases with the intensity of the preexposed stimulus (Crowell & Anderson, 1972; Schnur & Lubow, Note 1) and with increases in time between preexposed stimuli (Lantz, 1973; Schnur & Lubow, Note 1). These relationships are also found in the conditioning preparation (Kimble, 1961).

The basic test of the conditioning postulates of CAT as an explanation of latent inhibition is straightforward. Attention should be maintained and latent inhibition minimized if the initial attention-like response is followed either by an unconditioned stimulus (as in the usual conditioning preparation) or by any environmental change. Conversely, a lack of change following the initial attention-like response would result in a decrement in attention and an increase in latent inhibition. Implicit in these statements is the general proposition of a multiprocess theory of learning where the first process is the conditioning of attention to the relevant stimuli, and where subsequent processes account for the associative bonding of those stimuli with the required response (cf. Sutherland & MacKintosh, 1971; Zeaman & House, 1963).

The experiments reported here were designed to test this conditioned attention theory of latent inhibition. Experiments 1 and 2 tested predictions derived from the conditioning portion of the theory. Experiments 3 and 4 were concerned with some informal suppositions involving the attentional aspects of the theory. Experiments 1 and 2 attempted to maintain the attention response to the preexposed CS (hereafter referred to as S_1) by following each presentation of S_1 with the presentation of a second stimulus, S_2, in a conditioning relationship to it. According to CAT, this should attenuate latent inhibition. Experiments 3 and 4 were based on the assumption that control of stimulus onset would increase attention to that stimulus and therefore reduce the amount of latent inhibition as compared to a control group that received the same number and distribution of stimulus preexposures but without control.

EXPERIMENT 1

Experiment 1 compared the relative effectiveness of the following conditions: (1) no preexposure to the to-be-conditioned stimulus, (2) preexposure to the to-be-conditioned stimulus where each preexposure is immediately followed by a second stimulus, (3) preexposure to the to-be-conditioned stimulus and to the second stimulus, but in an unpaired relationship, and (4) preexposure only to the to-be-conditioned stimulus.

Using a CER technique, the general predictions were that the greatest suppression to S_1 would be exhibited by the nonpreexposed group with the least suppression exhibited by the group exposed to the to-be-conditioned stimulus alone. The amount of suppression to S_1 in the paired preexposure group should be raised by the addition of S_2 in temporal continguity with S_1. The addition of S_2 with the absence of temporal contiguity should not have the effect of increasing suppression as compared to the S_1-alone group.

Method

Subjects. The subjects were 96 male albino Charles River rats obtained from the Tel-Aviv University animal laboratory at approximately 70 days of age. They were housed one to a cage under reverse cycle lighting for the duration of the experiment.

Apparatus. The apparatus consisted of two plywood test chambers set in a ventilated sound-insulated Grason-Stadler research chest (Model 1101). The internal dimensions of each chamber were 15 × 20 × 17 cm as measured from the raised grid floor. The chambers were flat black, with small holes drilled in the side for ventilation. The house light was provided by a 1.5-W 12-V lamp attached to the chamber ceiling.

A Richter tube could be inserted into the cage through a 2-cm diameter hole which was 1.2 cm above the grid and 3 cm from the right side of the cage. When the Richter tube was not present, the hole was covered by a plastic lid. Licks were detected by a drinkometer circuit and recorded on an electronic counter.

The preexposed to-be-conditioned stimulus (S_1) was either a 3-sec tone or a 3-sec light. The second stimulus (S_2) was of 1-sec duration. If S_1 was a tone then S_2 was a light, and vice versa.

The tone was a 2.8-kHz signal produced by a Sonalert module (Model SC628). The nominal intensity of 72-db. sound pressure level (SPL) (re 20 $\mu N/m^2$) was reduced by placing a cardboard insert over the vibrator. The light was provided by a 3-W 12-V lamp attached to the center of the front wall of the chamber. The shock grid was made from stainless steel bars .25 cm in diameter, mounted at 1.5-cm intervals. Shock was supplied by a Grason-Stadler scrambled shock source (Model E106465P) set at 1 mA.

Procedure. Upon delivery, subjects were maintained on ad-lib food and water for 1 week. During the next 7 days, subjects were placed on a 23-hr water deprivation schedule which continued throughout the experiment. They were randomly assigned to one of four major groups.

Group 1 received no preexposure to the to-be-conditioned stimulus (S_1) nor to S_2. Group 2 was preexposed to S_1 and S_2 for 60 trials, 20 per day. The offset of S_1 was immediately followed by S_2. Group 3 received the same number of preexposures to S_1 and S_2, but S_2 was not temporally contiguous with S_1. S_2 appeared randomly, with the restriction that it did not occur within 10 sec of onset or offset of S_1. Group 4 received the same prexposure conditions as Groups 2 and 3, but only to S_1.

Animals from all groups received the preexposure condition for 20 min per day on Experimental Days 1, 2, and 3. The intertrial interval for Groups 2, 3, and 4 was variable with a mean of 60 sec and a range of 40–80 sec as measured from S_1 onset to the following S_1 onset.

For half of the subjects in each major group S_1 was the tone and S_2 the light, while for the other half of the subjects S_1 was the light and S_2 was the tone. All subjects received their 1-hr water ration in the home cage within ½ hr after being removed from the experimental apparatus.

After the preexposure condition ended on Day 3 all animals were treated identically. On Day 4 the tapwater-filled Richter tubes were introduced into the test chamber. Each subject was placed into the chamber and allowed to make 100 licks. The intervals between Licks 1 and 90 and Licks 90 and 100 were recorded. Richter tubes were refilled before the testing of each animal. On Day 5 the Richter tubes were removed. The subject was placed into the apparatus and after 5 min received a 3-sec S_1 (either tone or light, S_1 in this phase being the same as S_1 in the preexposure phase) immediately followed by a 1-sec 1-mA shock. After an additional 5 min subjects were returned to the home cages and allowed access to water for 1 hr.

On Day 6 the Richter tube was replaced in the test chamber. Each subject was placed in the apparatus and upon completion of 90 licks received S_1. S_1 continued until 10 more licks were completed. If the subject failed to complete the last 10 licks within 300 sec, the session was terminated and his score recorded as 300. The intervals between 1–90 licks without S_1 and 90–100 licks with S_1 were recorded.

The major dependent variable concerns the last measure. In order to take into account differences in response rate between subjects that were independent of the experimental treatment, the measure was expressed in terms of a suppression ratio $A/A+B$, where A is the amount of time to complete licks 90–100 on Day 4, and B is the amount of time to complete licks 90–100 on Day 6 during the presence of the CS. The lower the suppression ratio the greater the amount of suppression, reflecting the strength of conditioning on Day 5 as influenced by the experimental treatments during preexposure. To the extent that these effects are stimulus specific all of the other dependent measures, recorded in the absence of the CS, should not yield significant differences.

Results and Discussion

The median times to complete Licks 1–90 in the absence of the CS for groups 1, 2, 3 and 4 on Day 4 and Day 6, respectively, were 19.3, 27.5, 20.8, and 25.5 sec; and 19.0, 18.5, 19.3, and 22.5 sec. In both cases a Kruskal-Wallis one way analysis of variance indicated no significant differences among the groups ($H(3) = 4.66,\ p > .10$, $\mathrm{H}(3) = 3.55,\ p > .20$, respectively). Thus, there is neither evidence for nonspecific effects of the treatment variables nor for sampling bias.

The median suppression ratios for the groups as a function of type of stimulus (tone or light) was .36 and .34, respectively. A Mann-Whitney U test, using the z sampling distribution, indicated no significant differences as a function of stimulus type ($z = .015,\ p > .48$). Therefore, all subsequent analyses were performed on the four major groups collapsed across stimulus type in each group.

The median suppression ratios for each of the four groups were as follows: Group 1 (0), .05; Group 2 (S_1–S_2), .35; Group 3 (S_1, S_2), .46; Group 4 (S_1), .47. Differences between groups were analyzed using the Mann-Whitney U test. All tests are one-tailed except where noted, and use the z sampling distribution for determining significance values.

The major predictions of this study were confirmed. First of all, nonreinforced preexposure of the to-be-CS as compared to no preexposure (Group 4 vs. Group 1) resulted in less suppression ($z = 5.80,\ p < .001$). This basic finding of LI is well documented in other CER studies as well as in many other learning paradigms (Lubow, 1973).

In addition, it was predicted from the conditioned attention theory of LI that the addition of a second stimulus in a conditioning relationship to the to-be-CS should attenuate the latent inhibition effect. That is, there should be more suppression in the S_1–S_2 paired group than in the group preexposed to S_1 only (Group 2 vs. Group 4). This prediction was also confirmed ($z = 2.13$, $p < .02$).

The experimental design also allows for an evaluation of the effects of the S_2 addition to preexposure independent of its association with S_1. The contribution of S_2 to the attenuation of the LI may be a result of associative or nonassociative properties. To the extent that Group 3 (S_1, S_2 unpaired) also showed an attenuation of LI, then that attenuation may be attributed to nonassociative factors. A comparison of the S_1, S_2 unpaired group with the S_1-alone group (Group 3 vs. Group 4) indicates that the differences in median suppression ratio are not significant ($z = .78$, $p > .40$, two-tailed). On the other hand, the S_1–S_2 paired group achieved more suppression than the S_1, S_2 unpaired group (Group 2 vs. Group 3) ($z = 1.54$, $p = .062$). Although the latter statistical comparison does not reach the conventional level of reliability, the differences are in the predicted direction, suggesting that the effect of S_2 is to reduce the amount of LI as a result of S_2 being in a conditioning relationship to S_1, rather than by the nonassociative effect resulting from the presence of S_2, alone.

Additional comparisons indicate greater suppression for Group 1 as compared to Group 3 ($z = 5.32$, $p < .001$), and for Group 1 as compared to Group 2 ($z = 4.15$, $p < .001$). The latter result suggests that although the pairing of S_2 maintains the strength of the attentional response as compared to presentations of S_1, there is a decrement in the attention response as compared to a completely novel S_1.

EXPERIMENT 2

The purpose of this experiment was to evaluate the replicability of the basic findings obtained in Experiment 1 under slightly modified conditions, and to evaluate further the difference between the S_1–S_2 and S_1, S_2 groups, a particularly critical comparison for the theory. As in Experiment 1, the relative effectiveness of the following conditions was compared: (1) no preexposure to the to-be-conditioned stimulus, (2) preexposure to the to-be-conditioned stimulus immediately followed by a second stimulus, (3) preexposure to the to-be-conditioned stimulus and to the second stimulus, but *independently* scheduled, and (4) preexposure to only the to-be-conditioned stimulus.

Method

Subjects. The subjects were 32 male albino rats of the Sprague-Dawley strain obtained from the Sprague-Dawley Company (Madison, Wisconsin). All rats were approximately 70 days of age and were housed two to a cage.

Apparatus. The apparatus consisted of two rat-test chambers (BRS-Foringer), each set in a light and sound attenuating chamber (BRS-Foringer). A drinking tube could be inserted into the cage through a .8-cm hole in the end wall which was 7.5 cm above the grid floor and 5.0 cm from the side of the cage. Licks were recorded by a drinkometer amplifier (BRS-Foringer). When indicated by experimental procedures, the tube was removed and the hole left uncovered.

The shock grid was made from stainless steel bars .6 cm in diameter, mounted at 2.0-cm intervals. Shock was supplied by a scrambled constant current source (BRS Model SGS-003) set at 2 mA. The light CS was provided by three stimulus "Q" lamps (1820 bulb, 1.27-cm lens diameter) mounted on the end wall 14.0 cm above the grid floor and 4.5 cm, 12.0 cm, and 19.5 cm from the other three sides of the cage. The tone CS was 1 kHz at an intensity of 91-db. SPL (re 20 $\mu N/m^2$). The tone was delivered through a speaker located in each enclosure. However, the intensity was measured from a location in the center of the chamber. A ventilator fan maintained a constant masking noise of 75-db. SPL (re 20 $\mu N/m^2$).

Procedure. Upon delivery from the supplier, rats were maintained on ad-lib food and water for approximately 1 week. Throughout the experiment food and water were continued ad lib except where contraindicated by the procedure.

The experimental procedure spanned 5 days. On Day 1, rats were weighed and water was removed from the home cages. On Day 2, each rat (24-hrs deprived) was placed in the apparatus and allowed to make 200–250 licks on the drinking tube, and baseline data were collected. The intervals between Licks 1–100 and Licks 100–110 were recorded. Water was made available in the home cages on an ad-lib basis following the Day 2 session, and the drinking tubes were removed from the chambers. At this stage, two animals in each replication were assigned randomly to each of the four preexposure groups previously described in Experiment 1. Group 1 received no preexposure to the to-be-conditioned stimulus (S_1) nor to S_2. Group 2 was preexposed to S_1 and S_2 for 120 trials, 60 per day, on a variable time (VT) 2-min schedule. The offset of S_1 was followed immediately by S_2. Group 3 received the same number of preexposures to S_1 and S_2, but the stimuli were presented on independent VT 2-min schedules. Thus, for Group 3 a small number of S_1–S_2 pairings may have occurred by chance. Group 4 received the same preexposure conditions as Groups 2 and 3, but only to S_1.

Animals from all groups received the preexposure condition for 2 hr per day on Days 3 and 4. For all subjects, S_1 was the light stimulus and S_2 was the tone stimulus. The duration of both S_1 and S_2 was 10 sec, and the ISI for Group 2 was 10 sec.

After the final preexposure on Day 4 (or after 2 hours on Day 4 for the nonpreexposed group), all subjects received two acquisition trials in which the light stimulus (S_1) was followed immediately by footshock. On these trials, the 10-sec light stimulus continued on a VT 2-min schedule, but now each presentation ended in a 1-sec footshock. The ISI on these trials was 10 sec. At the end of the Day 4 session, water was again removed from the home cages. On Day 5, each subject was placed in the apparatus and after completion of 100 licks, the light stimulus was presented. Light presentation continued until the subject completed 10 additional licks. If the subject failed to complete the last 10 licks within 300 sec, the session was terminated and the score was recorded as 300 sec. Times to complete Licks 1–100 and Licks 100–110 were recorded. The complete experimental design was replicated four times on successive weeks, thus making 8 subjects per group.

A suppression ratio, $A/A+B$, similar to that used in Experiment 1, was calculated for each subject: A is the amount of time to complete Licks 100–110 on Day 2, and B is the amount of time to complete Licks 100–110 on Day 5 in the presence of the CS.

Results and Discussion

The median times to complete Licks 1–100 in the absence of the CS for Groups 1, 2, 3, and 4 on Day 2 and Day 5, respectively, were 18.5, 27.2, 19.4, and 18.2 sec; and 81.0, 47.6, 45.0, and 60.6 sec. In both cases, a Kruskal-Wallis one way analysis of variance indicated no significant differences among the groups ($H(3) = 1.65$, $p > .50$; $H(3) = 1.26$, $p > .75$, respectively). As in Experiment 1 there is neither evidence for nonspecific effects of the treatment variables nor for sampling bias.

The median suppression ratios for each of the four groups were as follows: Group 1 (0), .02; Group 2 (S_1–S_2), .05; Group 3 (S_1, S_2), .30; Group 4 (S_1), .25. Differences between groups were analyzed using the Mann-Whitney U test. All tests are one-tailed except where noted.

The major findings of Experiment I were replicated. First of all, nonreinforced preexposure of the to-be-continued stimulus as compared to no preexposure resulted in lower suppression ($U = 6$, $p < .002$). In addition, it was predicted from the conditioned attention theory that the addition of a second stimulus in a conditioning relationship to the to-be-conditioned stimulus would attenuate the latent inhibition effect. That is, there should be more suppression in the S_1–S_2 paired group than in the group preexposed to S_1 alone (Group 2 vs. Group 4); this effect was shown ($U = 12$; $p < .019$). Moreover, the difference between the nonpreexposure group and the S_1–S_2 paired group was not significant ($U = 20$, $p > .24$, two-tailed). In the present experiment, therefore, conditioning to a CS which had been followed by a second stimulus during preexposure was equivalent to conditioning to a novel stimulus.

The experimental design also allows for an evaluation of the effects of the S_2 addition to preexposure independent of its association with S_1. According to CAT, this manipulation should not attenuate latent inhibition. Group 3 (S_1, S_2 independently presented) also showed little suppression. The difference between Group 3 and Group 1 (no preexposure) was significant ($U = 9$, $p < .007$), while that between Group 3 and Group 4 (S_1 alone) was not ($U = 30$, $p > .88$, two-tailed).

Thus, the same effect was obtained when a second stimulus was preexposed independently of S_1 (Group 3) as well as when S_1 was preexposed alone (Group 4). A final test of the effects of adding a second stimulus during preexposure either in a conditioning relationship to S_1, or independently of S_1, can be seen in the median suppression ratios of Groups 2 and 3. The data show that Group 2 (S_1–S_2) achieved more suppression than Group 3 (S_1, S_2), but, again, this difference was marginally significant ($U = 17$, $p < .065$). However, combining the independent significance levels from Experiments

1 and 2 yields a reliable difference ($z = 2.16$, $p < .015$; Mosteller & Bush, 1954, p. 329).

In general then, Experiment 2 confirms the reliability of the results of Experiment 1, and adds to their generalizability by obtaining similar results even with procedural changes. The ordering of groups for which predictions were made was the same in both experiments. In fact the only groups that reversed their relative positions between experiments, Groups 3 and 4, were those for which no directional predictions were made. Also to be noted is that there was a considerable difference in the overall amount of suppression between Experiments 1 and 2, with considerably greater suppression in Experiment 2. This interexperiment difference is attributable to the fact that Experiment 1 used one trial of acquisition with a 1-mA shock, while Experiment 2 employed two trials of acquisition with a 2-mA shock. In spite of the overall differences in suppression, the predictions from CAT still were confirmed.

It should be noted that the conditions of Experiments 1 and 2 do not allow an assessment of the latent inhibition effect (i.e., decreased associability to the preexposed stimulus) independent of habituation of disruptive effects of a novel stimulus on licking. In other words, the differential suppression between groups may have been due to the differential unconditioned effects of the stimuli (differential habituation) as opposed to, or as well as, differential acquistion of the CER (differential latent inhibition). Although this is true, there is evidence to indicate that the former effect is relatively small compared to the latter (Lubow & Siebert, 1969).

The pattern of results achieved in these two experiments is also subject to other interpretations and do not uniquely confirm CAT. For example it is possible that the augmented suppression for the S_1–S_2 preexposed groups was produced as a result of *omission* of S_2 during acquisition and testing. This would have created a greater change, relative to the preexposure conditions, for this group as compared to the S_1 or the S_1, S_2 unpaired groups. This possibility must be entertained in view of the fact that test responding occurred in the presence of S_1 during and after the time that S_2 would have been presented in the S_1–S_2 groups during preexposure. However, in a similar study pertaining to differences in habituation, in which the differential response to S_1 was measured *prior* to the usual time of occurrence of S_2, the same pattern of results as in our study was achieved (Wagner, in press). Indeed, Wagner's theory of habituation is equally capable of predicting the data reported in Experiments 1 and 2.

EXPERIMENT 3

If an assumption is made that the subject's control of a stimulus results in that stimulus eliciting more of an attentional response than when there is lack of control, or passive reception of the stimulus, then, according to conditioned attention theory, the amount of latent inhibition demonstrable to such a preexposed controlled stimulus should be relatively low compared to a comparable uncontrolled stimulus. It should be noted that the assumption relating controllability to attention is not central to the conditioned attention theory. Nevertheless, the assumption is plausible and led to the two experiments reported in the following section.

Method

To test this hypothesis, a pair of rats was yoked in adjoining Skinner boxes, so that a bar press in Box A produced a tone both in Box A (active control) and Box B (passive reception), while bar press in Box B had no such consequence. With the greater R_A there should be less LI in the active stimulus control group than in the passive group. It was predicted that the active preexposed group would exhibit more suppression than the passive preexposed group, and that the nonpreexposed group would exhibit more suppression than the passive preexposed group.

Subjects. The subjects were 66 male Charles River albino rats obtained from the Tel-Aviv University animal laboratory at approximately 70 days of age. They were individually housed under reverse cycle lighting conditions for the duration of the experiment.

Apparatus. The apparatus was the same as that in Experiment 1. It was modified by adding a manipulandum (2.5 × 4 cm) located 2.5 cm from the left side of the cage, 2 cm above the grid. Bar presses were recorded on an electromechanical counter.

Procedure. Upon delivery, subjects were maintained on ad-lib food and water for 1 wk. During the following 7 days, subjects were on a 23-hr water deprivation schedule which continued throughout the experiment. They were randomly assigned to one of three major groups: active, passive, or control. For half the animals in the first two groups the right box was active, and for the other half the left box was active. The order in which the active–passive and control pairs were run was counterbalanced. On Day 1 of the experiment, each pair was placed in the apparatus for a 30-min preexposure session. The active–passive pairs were yoked so that when the active subject pressed the manipulandum the tone was activated for 3 sec in both chambers. When the passive subject pressed the manipulandum, the tone was not activated in either chamber. Bar pressing by the control subjects did not activate the tone.

The number of bar presses and the number of CS preexposures for each group were recorded. At the end of each preexposure session, subjects were returned to their home cages where water was available for 1 hr. There was one preexposure session daily for 3 consecutive days. On Day 4, tapwater-filled Richter tubes were introduced into the test chambers and the manipulanda were disconnected. Each subject was placed in the chamber and allowed to make 100 licks. The intervals between Licks 1–90 and Licks 90–100 were recorded. At the end of the session, subjects were returned to their home cages where water was available for 1 hr. On the 5th day, subjects were placed in the apparatus and after 60 sec received a 3-sec tone immediately followed by a 3-sec 1-mA shock. The Richter tubes were absent from the chambers. Subjects were then returned to the home cages and allowed access to water for 1 hr.

On the 6th day, the Richter tubes again were made available in the test chamber. Each subject was placed in the chamber, and, after completion of 90 licks, the tone was presented. The tone continued until the subjects completed 100 licks. If the subject failed to complete the last 10 licks within 300 sec, the session was terminated and the score was recorded as 300. Times from 1–90 licks without tone and 90–100 licks with the tone were recorded.

Results

The median number of bar presses per group for each of the 3 days of preexposure is shown in Figure 1. Although the stimulus duration was 3 sec, there was no bar pressing during that stimulation. Thus number of bar presses for the active group also describes the number of preexposures for both the active and passive groups. The total number of stimulus preexposures ranged from 6–61, with the median number 18. There were no significant differences among the three groups (Kruskal-Wallis, $H = .26$, $p > .50$).

The median times to complete Licks 90–100 on Day 4 and Licks 1–90 on Day 6, respectively, for the active, passive, and control groups were 1.55, 1.95, and 2.25 sec; and 20.75, 19.50, and 25.88 sec. There were no significant differences among groups for either measure (Kruskal-Wallis, $H = 3.32$, $p > .10$; $H = 1.17$, $p > .50$, respectively).

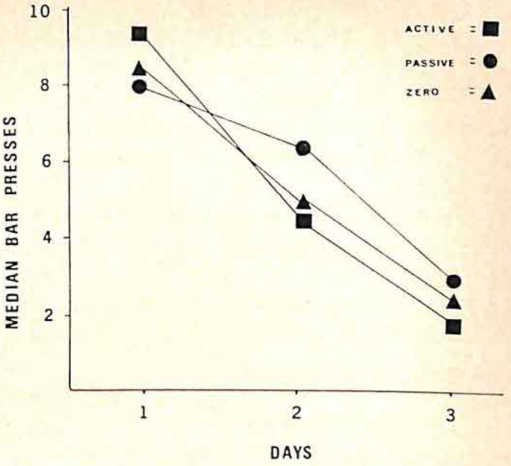

FIGURE 1. Median number of bar presses for active-, passive-, and zero-preexposure groups as a function of days, Experiment 3.

The major dependent variable is expressed in terms of a suppression ratio, $A/A + B$, as described in Experiment 1. The first hypothesis predicted less suppression for the passive preexposure group than the no preexposure group. The median suppression ratios for these two groups were .48 and .05, respectively (see Table 1). Using the Wilcoxon matched-pairs signed ranks test, and pairing animals on the basis of being run in the same replication and the same test chamber, these differences were found to be significant ($T = 22$, $p < .005$, one-tailed).

TABLE 1

MEDIAN SUPPRESSION RATIOS FOR ACTIVE PREEXPOSED GROUPS (A), PASSIVE PREEXPOSED GROUPS (P) AND NONPREEXPOSED GROUPS (N), EXPERIMENTS 3 AND 4

Group	Experiment	
	3	4
A	.25	.05
P	.48	.40
N	.05	.02

The second hypothesis predicted more suppression for the active preexposed animals as compared to their yoked passive partners. The respective median suppression ratios for these two groups, .25 and .48, were also reliably different ($T = 22$, $p < .025$, one-tailed).

The active preexposure group and the no preexposure group were significantly different from each other ($T = 24$, $p < .01$, two-tailed). The former showed less suppression than the latter, .25 and .05, respectively.

EXPERIMENT 4

This study employed the same experimental design as Experiment 3 but with a light as conditioned stimulus, and with a number of procedural differences. The unusual nature of the result exhibited in Experiment 3 in regard to the absence of differences in bar-press rate between the three groups in spite of the subsequent predicted differences between these groups made it worthwhile to have additional checks on the generality of the phenomenon.

Method

Subjects. The subjects were 24 male Sprague-Dawley albino rats supplied by the Holtzman Company. All rats were between 65 and 76 days of age at the start of the experiment.

Apparatus. The apparatus consisted of two rat-test chambers (BRS-Foringer), each set in a light and sound attenuating chamber. A drinking tube could be inserted into the cage through a .8-cm hole which was 7.5 cm above the grid and 5.0 cm from the side of the cage. Licks were recorded by a drinkometer amplifier (BRS Model DO-201). When indicated by experimental procedures the tube was replaced by a manipulandum (2.6×3.5 cm). Each chamber contained a food hopper 12.5 cm from the side of the end wall. In one chamber the drinking tube and manipulandum were located to the right of the food hopper; in the other they were located to the left.

The shock grid was made from stainless steel bars .6 cm in diameter, mounted at 2.0-cm intervals. Shock was supplied by a scrambled constant current source BRS Model SGS-003) set at 2 mA. The CS was provided by a houselight mounted in the chamber behind frosted glass on the top of the wall opposite the intelligence panel. When the CS was not activated the chamber was dark. A ventilator fan maintained constant masking noise of 75-db SPL (re 20 $\mu N/m^2$).

Procedure. Upon delivery, subjects were individually caged and maintained on ab-lib food and water. On Day 1 of the experiment, subjects were placed on a 23-hr water deprivation schedule. On Day 2, each rat was placed in the apparatus and allowed to make 150–200 licks. This was repeated on Day 3, and baseline data were collected. The times between Licks 1–100 and Licks 100–110 were recorded. Water was made available in the home cages on an ad-lib basis following the Day 3 session. On Day 4, the manipulandum replaced the drinking tube in the apparatus. At this stage, animals were assigned to one of three preexposure groups: active, passive, and control. Each animal in the passive group was run with a partner from the active group. On Day 4, each active–passive pair was placed in the apparatus for a 25-min preexposure session. The pairs were yoked so that when the active subject pressed the manipulandum the houselight CS was activated for the duration of the bar press in both chambers. When the passive subject pressed the manipulandum the houselight was not activated in either chamber. Control pairs received similar treatment but bar pressing did not activate the houselight. No CS was presented for these subjects. There was one preexposure session daily for 4 days. On Day 7, following the final preexposure trial and after 25 min of the session (or simply after 25 min for the no preexposure group) a single noncontingent CS–US pairing was presented. All subjects received a 5-sec houselight CS immediately followed by a 1-sec 2-mA shock. All rats were removed from the apparatus following this trial. Water was removed from the home cages at this time. On Day 8, each subject was placed in the apparatus, and after completion of 100 licks the houselight was presented. Houselight presentation continued until the subjects completed 10 additional licks. If the subject failed to complete the last 10 licks within 300 sec, the session was terminated and the score was recorded as 300. Times from 1–100 licks without houselight and 100–110 licks with houselight were recorded.

Results

The median number of bar presses per group for each of the 4 days of preexposure is shown in Figure 2. Since the bar press controlled stimulus onset and offset, the number of bar presses for the active group also describes the number of preexposures for both the active and passive groups. The total number of stimulus preexposures ranged from 23–99, with the median number 52. There were no reliable differences among the three groups (Kruskal-Wallis, $H(2) = 1.19$, $p > .50$).

The median times to complete Licks 100–110 on Day 3 and Licks 1–100 on Day 8, respectively, for the active, passive, and control groups were 1.65, 2.30, and 2.35 sec; and

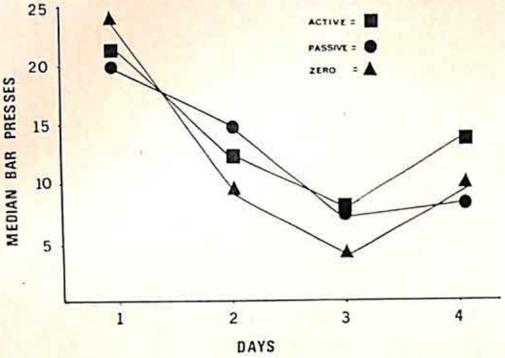

FIGURE 2. Median number of bar presses for active-, passive-, and zero-preexposure groups as a function of days, Experiment 4.

39.30, 26.05, and 57.10 sec. There were no significant differences among groups for either measure (Kruskal-Wallis, $H(2) = 4.17$, $p > .10$; $H(2) = 2.00$, $p > .30$, respectively).

The major dependent variable is expressed in terms of a suppression ratio, $A/A + B$, as described in Experiment 1. The first hypothesis predicted less suppression for the passive preexposure group than the no preexposure group. The median suppression ratios for these two groups were .40 and .02, respectively (see Table 1). Using the Wilcoxon matched-pairs signed ranks test, and pairing the animals on the basis of being run in the same replication and the same test chamber, these differences were found to be significant ($T(7) = 0$, $p < .01$, one-tailed). The second hypothesis predicted more suppression for the active preexposed animals as compared to their yoked passive partners. The respective median suppression ratios for these two groups, .05 and .40, were also reliably different ($T(7) = 2$, $p < .025$, one-tailed). The median suppression ratios for the active preexposure group and the no preexposure group were .05 and .02, respectively, and these did not differ significantly ($T(7) = 6$, $p > .10$, two-tailed).

Discussion

The results of Experiments 3 and 4 are very consistent. This is particularly noteworthy since the designs, although basically similar, were executed with a number of procedural variations between experiments.

Both studies found a decrease in bar pressing as a function of number of days in the apparatus. Further, in both studies there were no significant differences in number of bar presses among active, passive, and no preexposure groups. These between-experiment consistencies were obtained in spite of the fact that in Experiment 3 the subjects received the stimulus preexposure treatment to a 3-sec tone for 3 days, 30 min per day, while in Experiment 4 the duration of the stimulus, a light was controlled by the duration of the bar press and administered over 4 days, 25 min per day.

In addition, the bar pressing phase of Experiment 3 was initiated on the 1st day of introduction to the apparatus. In Experiment 4, this phase was preceded by 2 days of apparatus familiarization. It is this latter fact that may account for the absolutely higher rates of bar pressing in Experiment 4 as compared to Experiment 3. A number of studies concerned with apparatus familiarity as the independent variable have demonstrated that preexposure to the apparatus facilitates the acquisition of a stimulus-contingent bar press (e.g. Williams & Lowe, 1970, 1972).

The second major similarity between results of the two studies concerns the predictions for the relative effects of active stimulus preexposure, passive stimulus preexposure, and no preexposure on the acquisition of conditioned suppression. It was predicted that the passive preexposed groups would exhibit poorer learning than the nonpreexposed group. This difference was highly reliable in both experiments, with the passive preexposed groups showing virtually no conditioning (suppression ratios close to .50) while the nonpreexposed groups showed almost complete conditioning (suppression ratios close to zero). The finding of poor learning as a result of passive preexposure is in concordance with the data on LI in general (Lubow, 1973), and specifically replicates similar findings within the CER paradigm (e.g. Carlton & Vogel, 1967; Lubow & Siebert, 1969). However, unlike previous studies of LI, the number, duration, and patterning of stimulus preexposures were not under experimenter control. The demonstration of LI under variable modes of stimulus preexposure (as produced by the yoking procedure) further extends the already broad conditions for producing the phenomenon.

The second prediction concerned the differences in amount of LI as a result of active preexposure as compared to passive preexposure. It was hypothesized that the active groups would better maintain the attentional response to the stimulus, which is contingent on bar pressing, than the passive group, and, therefore, would show better subsequent conditioning to that stimulus. This prediction was also confirmed in both experiments. The active preexposed groups showed more suppression than the passive preexposed groups. The above prediction was confirmed even though the groups did not differ in bar-press rate during the preexposure phase. It might be expected that the active group would exhibit a sensory reinforcement effect (Kish, 1966), that is, a greater number of responses than those groups in which S_1 presentation was not contingent on bar press. That latent inhibition differences were manifest in the subsequent testing phase, even though bar-press differences were not achieved in the preexposure phase, suggests that the former test is more sensitive to the effects of response-contingent stimulation than the latter testing procedure.

GENERAL DISCUSSION

The results from Experiments 1 and 2 conformed to the expectations derived from the conditioning assumptions of CAT. LI is attenuated when a second stimulus is placed in a conditioning relationship with the preexposed stimulus. The results show that the effectiveness of S_2 is a result of its temporal contiguity with S_1 and thus supports a conditioning interpretation of the decremental effects of preexposure to S_1 alone on subsequent learning.

These data confirm and parallel the findings of Lubow, Alek, and Arzy (1975). In their study, children were tested in a simple reaction-time situation. Some groups were passively preexposed to the stimulus to be used in the subsequent test (S_1), other groups were passively preexposed to S_1 immediately followed by S_2. It was found, as predicted by CAT, that not only did reaction time increase with the number of preexposures, but that this function was significantly attenuated by introducing S_2 during preexposure in a conditioning relationship to S_1. In addition, there was some suggestion that the interstimulus interval between S_1 and S_2 may affect the amount of attenuation of LI, with .5 sec being more effective than 5.0 sec.

The major results of Experiment 1, 2, 3, and 4 also may be interpreted in a different manner. Taking the active preexposed versus the yoked passive preexposed groups as an example, perhaps both preexposure procedures equally reduce attention to the stimuli encountered. However, differences in subsequent conditioning to a nominal CS may result from the *change* in mode of CS presentation from preexposure to aquisition trials. This change exists only for the active preexposure group. That is, for passive preexposure animals, CS presentation on the acquisition trial is response independent as it was on the previous preexposure trials. For active preexposure animals, on the other hand, the response independent CS presentation during acquisition is in contrast to the response correlated CS presentations during preexposure. Subsequent differences in conditioning performance would then be attributable to differences in stimulus salience induced by the *transition* between preexposure and acquisition (cf. Lubow, Rifkin, & Alek, 1976). Analogous reasoning with respect to Experiments 1 and 2 has already been acknowledged. The principal finding that S_1–S_2 paired exposures produced less latent inhibition than S_1, S_2 unpaired exposures may be attributable to the fact that the measurement interval involved the time that S_2 would normally occur in the former condition but not in the latter, and thus involved differential stimulus change in the two cases.

In regard to the differences between active and passive preexposure, this explanation suggests that there is an equal reduction in stimulus salience for both groups with increased number of preexposures, but that the original salience is restored to the active group because of the change in conditions from preexposures to acquisition. No such change is found in the passive group and therefore stimulus salience remains low. This dishabituation or external inhibition type of explanation is not necessarily antithetical to the proposed CAT. Conditioning may still be the process by which salience reduction occurs during preexposure. What is added is another conditioning construct (external inhibition) to account for the better performance of the active group over the passive group.

An important attribute of CAT is the ease with which testable predictions can be generated. Generally, any manipulation which enhances Pavlovian conditioning should, if applied to the preexposure phase of the LI paradigm, enhance conditioned inattention and thus decrease subsequent learning. From this a number of predictions emerge: (a) The greater the intensity of the preexposed stimulus the greater the latent inhibition. This has been confirmed (Crowell & Anderson, 1972; Schnur & Lubow, Note 1). (b) The longer the intertrial interval during preexposure the more LI there will be. This too has been confirmed (Lantz, 1973; Schnur & Lubow, Note 1). (c) Finally, as found in the present study and an earlier one (Lubow, Alek, and Arzy, 1975), a second stimulus placed in a conditioning relationship to the preexposed stimulus maintains the attentional response to that stimulus and thus attenuates the decremental effects of preexposure.

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Unconditional Stimulus Characteristics in Rabbit Eyelid Conditioning

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Three experiments employed aversive or positive subcortical electrical stimulation in an unconditional stimulus (US) role in rabbit eyelid conditioning. Conditional response (CR) development to a tonal conditional stimulus (CS) was examined when the intracranial stimulation was combined with a conventional cheek-shock US (Experiment 1) or with a light-flash US (Experiment 2). Our findings were consistent with previous observations that the affective impact of the US is important in the development of an overt motor CR. The complex pattern of our results, however, implies that intracranial stimulation has multiple effects on conditioning and argues against any unidimensional interpretation of these outcomes. Our analysis of CR-contingent intracranial stimulation presentation in Experiment 3 indicated that such stimulation does not act as a Thorndikian reward or punishment in rabbit eyelid conditioning.

Research investigations have indicated that reliable reflex elicitation is not sufficient by itself to ensure that a stimulus will be effective as a Pavlovian unconditional stimulus (US). Young (1954) and Gerall, Sampson, and Boslov (1957) failed to condition pupillary constriction when the US was a rapid change in illumination, even though conditioning was observed when an electric shock was employed as the US. Bruner (1965) demonstrated that a bright light flash, which reliably produced an eyeblink reflex in the albino rabbit, was an ineffective US for establishing conditioned eyeblinks to an auditory conditional stimulus (CS). Colavita (1965) demonstrated that an acetic acid solution delivered directly into the stomach of a dog reflexly produced salivation but was ineffective for establishing a salivary conditional response (CR) to an auditory CS. Wagner, Thomas, and Norton (1967) reported that electrical stimulation of the motor cortex, which reliably elicited limb movement, was ineffective in promoting observable responding to a CS when unconditional response (UR) occurrence was not accompanied by postural imbalance. In each of these studies, the ineffective US reliably elicited the appropriate response. In contrast to effective USs, however, the stimuli in these studies had little, if any, emotional impact.

The present experiments investigated US characteristics in conditioning by artificially eliciting an affective response with subcortical electrical stimulation. A tonal CS was paired with intracranial stimulation (ICS) or with a combination of an ICS and a more conventional peripheral stimulus, such as a cheek shock or a bright light flash. The purpose of these manipulations was to determine the associative effect of CS–US pairings when these unusual stimuli were employed as USs.

EXPERIMENT 1

Bruner (1966) coupled behaviorally positive or aversive brain stimulation with a more conventional cheek-shock US in order to develop a conditioned eyeblink response in the rabbit. The CS was a 70-db. (re 20 $\mu N/m^2$)

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buzzer, and the cheek shock was produced by the discharge of a .1 μF capacitor. The onset of a 500-msec train of brain stimulation occurred simultaneously with the onset of the cheek shock. Using a 90-sec intertrial interval and a 500-msec CS–US interval, Bruner observed that the brain stimulation produced an impressive facilitation of conditioning. Eight animals which had stimulation of positive sites coupled with the cheek-shock US reached an acquisition criterion in an average of 37 trials. Four rabbits receiving aversive brain stimulation in conjunction with the cheek shock reached the acquisition criterion in an average of 46 trials. Five implanted controls reached the criterion in an average of 91 trials. This outcome was remarkable not only in the size of the experimental effect but also because aversive and rewarding brain stimulation had essentially identical effects.

Our first experiment was undertaken in an attempt to replicate Bruner's findings while including two important comparison groups which received brain stimulation during the intertrial interval. It is possible that the brain stimulation employed in Bruner's study had a general arousing effect which facilitated performance irrespective of the stimulation's affective polarity. If arousal is the important factor, presentation of aversive or positive ICS during the intertrial interval should be just as effective as coupling the ICS with US presentation.

Method

Subjects. Data are reported for 48 male Dutch rabbits, aged 12–20 weeks, that weighed 1.5–2.0 kg. A twisted-wire electrode was implanted in the brain of each animal; the 48 subjects represent 16 positive sites, 16 aversive sites, and 16 implanted, nonstimulated controls (primarily neutral or ambiguous sites). The animals were caged individually and had free access to food and water. The vivarium was maintained on an artificial lighting cycle with lights on from 8 a.m. to 12 midnight each day.

Apparatus. The conditioning equipment employed in the present study was similar to that described in Frey and Gavin (1975). The rabbits were restrained within a Plexiglas and aluminum stock which permitted limb movement and postural changes but prevented escape from the conditioning situation. The restrained animals were placed in a ventilated and sound-attenuated walk-in cooler which was maintained at 20°C (± 1°) and 50% relative humidity (± 10%). As many as six animals could be conditioned concurrently in the darkened chamber. This chamber (2.6-m long, 2.0-m wide, and 2.1-m high) was illuminated by a single 7.5-W red exit light. The background sound level was maintained at 70db. by a white noise source (Grason-Stadler, Model 901B).

Eyelid movements were graphically recorded with a counterweighted minitorque potentiometer (Conrac Corp., Model 85153) and a 6-channel oscillograph (Beckman Model 5040). The US was the discharge of a .1-μF capacitor, charged to 200 V dc, across the two stainless-steel cheek electrodes. Both sides of the capacitor were switched from the charging circuit to the animal during US presentation to prevent any ground circuits from interacting with the concurrent intracranial electrical stimulation. This brief (several milliseconds) electric shock produced reliable eyelid closure with a latency of approximately 40 msec after US onset. The intracranial electrical stimulation was produced by a commercial square-wave isolation unit (Biomedical Electronics, Model S104B). The stimulation consisted of 100 biphasic pulse pairs each second (1 msec positive, 1 msec negative, 8 msec neutral). The peak-to-peak intensity of this stimulation was set individually for each animal and ranged from 140 μA to 400 μA. The brain stimulation was always a fixed duration of 500 msec. The CS was a 1,200-Hz tone, 12 db. above the background noise level as measured at the level of the rabbit's ears (General Radio sound-level meter, Type 1565-A, Scale Bs). This tone was presented for 800 msec via separate speakers which were located directly in front of each rabbit. The CS–US interval was 500 msec and the intertrial interval was 90 sec for single-cue conditioning and 45 sec for differential conditioning. In the coupled conditions, the onsets of the US and the ICS were simultaneous. In differential conditioning, CS+ (1,200 Hz) was followed by the US either coupled or unpaired with ICS, and the CS− (1,000 Hz) was presented without being followed by either the US or the ICS.

Procedure. After delivery by a local supplier (Scientific Small Animals, Arlington Heights, Illinois), the rabbits were habituated to their new home cages for at least 3 days prior to surgery. Each rabbit was subsequently anesthetized with pentobarbital and ether and surgically prepared by the introduction of skull bolts, a subcortical electrode, and cheek electrodes. The two stainless-steel skull bolts (2–56, 12.7 mm) penetrated the skull (one behind and to the animal's left of the bregma, and one in front to the animal's right of the bregma) and rested on the dura. The subcortical bipolar electrode was made by twisting two strands of chrome–nickel wire (.25 mm in diameter) together and anchoring them to a small plastic pedestal (supplied by William E. Wetzel, Chelsea, Michigan). The electrode was insulated with Formvar except for ½ mm at its tip. This electrode was implanted using a stereotaxic device manufactured by the Narishige Scientific Instrument Laboratory. Two lengths of 26-gauge suturing wire were attached below and to either side of the right eye. A rectangular tab of Velcro fastener (American Thread Co.) was attached to the right eyelid using Davol surgical appliance adhesive.

Ten days after surgery, each rabbit was tested for self-stimulation behavior in an operant chamber (58 cm × 48 cm). A 500-msec train of biphasic square-way stimulation (1 msec positive, 1 msec negative, 8 msec neutral) could be activated by moving an omnidirectional lever (Lehigh Valley Electronics, 121–21) which was located 6 cm above the chamber floor. The rabbits usually moved this lever by muzzle contact rather than paw contact. The current level varied from 140 to 400 μA peak-to-peak and was adjusted for each animal at the beginning of each session. Intensity level was determined by either observable reactions or by past history. Positive sites were defined in terms of 15 or more bar presses per minute during each of several 20-min. sessions. Several animals showed reliable rates as high as 70 presses/min. Aversive sites were defined in terms of the absence of self-stimulation behavior and by the appearance of overt signs of negative affect, such as foot thumping, vocalization, escape reactions, and pupillary and breathing changes. Neither positive nor aversive stimulation produced reliable eyelid responses. The present study reports conditioning data for 16 rabbits which met the criterion for a positive site and 16 rabbits which met the criterion for an aversive site. Sixteen other animals which failed to bar press and showed no overt signs of aversiveness were used as implanted (but nonstimulated) controls.

After each animal had been tested for self-stimulation behavior (generally 7–10 days), the animal was introduced to the conditioning environment in the following manner: 15–30 min. in the open stock, restraint in the stock for 30 min., and then a final 30 min. with all recording and stimulation gear in place. The following day, conditioning trials were initiated. In the first phase (Days 1–4), each animal received 60 trials per day at a 90-sec intertrial interval. The implanted controls received the 1,200-Hz tonal CS paired with the cheek shock at a 500-msec CS–US interval. Half of the positive-site animals and half of the aversive site animals received a 500-msec train of brain stimulation coupled with the US. The remaining positive and negative site animals received the 500-msec train of brain stimulation during the intertrial interval, 25 sec after the previous trial.

During the second phase of conditioning (Days 5–7), each animal received 120 differential conditioning trials each session at a 45-sec intertrial interval. CS+ trials (1,200-Hz tone) were alternated with CS− trials (1,000-Hz tone). For each of the subcortical stimulation groups, brain stimulation was either coupled with the US on CS+ trials or was presented 20 sec after each CS+ trial as during initial conditioning. The CS–US interval during this second phase was set at 600 msec (see Frey, 1969). After completion of the conditioning training, each animal was retested for self-stimulation behavior or for aversive effects. Two animals were replaced because their electrodes became nonfunctional sometime during the conditioning sessions. Subsequently, each animal was perfused with a Formalin solution, the brain was removed, frozen with CO_2, and then sliced at 50-micron intervals with a microtome (American Optical Corp., Model 880). At approximately 200 micron intervals the slices neighboring the electrode tip location were mounted on microscope slides and photographed while wet.

Results

Histology. The sites of the electrode tips for the animals which displayed self-stimulation rates of 15 bar presses/min. or higher are depicted in Figure 1. The coordinates and structure depiction are those published by Sawyer, Everett, and Green (1954). Seven of the positive sites were located in the lateral hypothalamus at the level where the fornix enters and turns caudally in the hypothalamus. These sites at P1 correspond closely to the major self-stimulation area depicted by Bruner (1967). The two positive sites at P2 also correspond to a self-stimulation area reported by Bruner. The remaining positive sites at APO and in the more medial locations at P1 do not correspond very closely to Bruner's mapping.

The aversive sites are also depicted in Figure 1. The five lateral sites at APO correspond closely to an aversive area depicted by Bruner (1967). The remaining sites seem to be somewhat scattered and do not coincide with aversive sites reported by Bruner. It is interesting that the two lateral sites at P1 are located in the same area which was highly positive for 7 other animals in the present study and for 14 of the animals reported by Bruner.

Conditioning data. Each trial was scored for the presence or absence of a conditioned eyelid response, defined as a movement of the recording pen in the appropriate direction of 1 mm or greater during the CS–US interval. The gain of the recording system was set such that a complete eyelid closure produced a pen movement of 20–25 mm.

The acquisition performance of the five

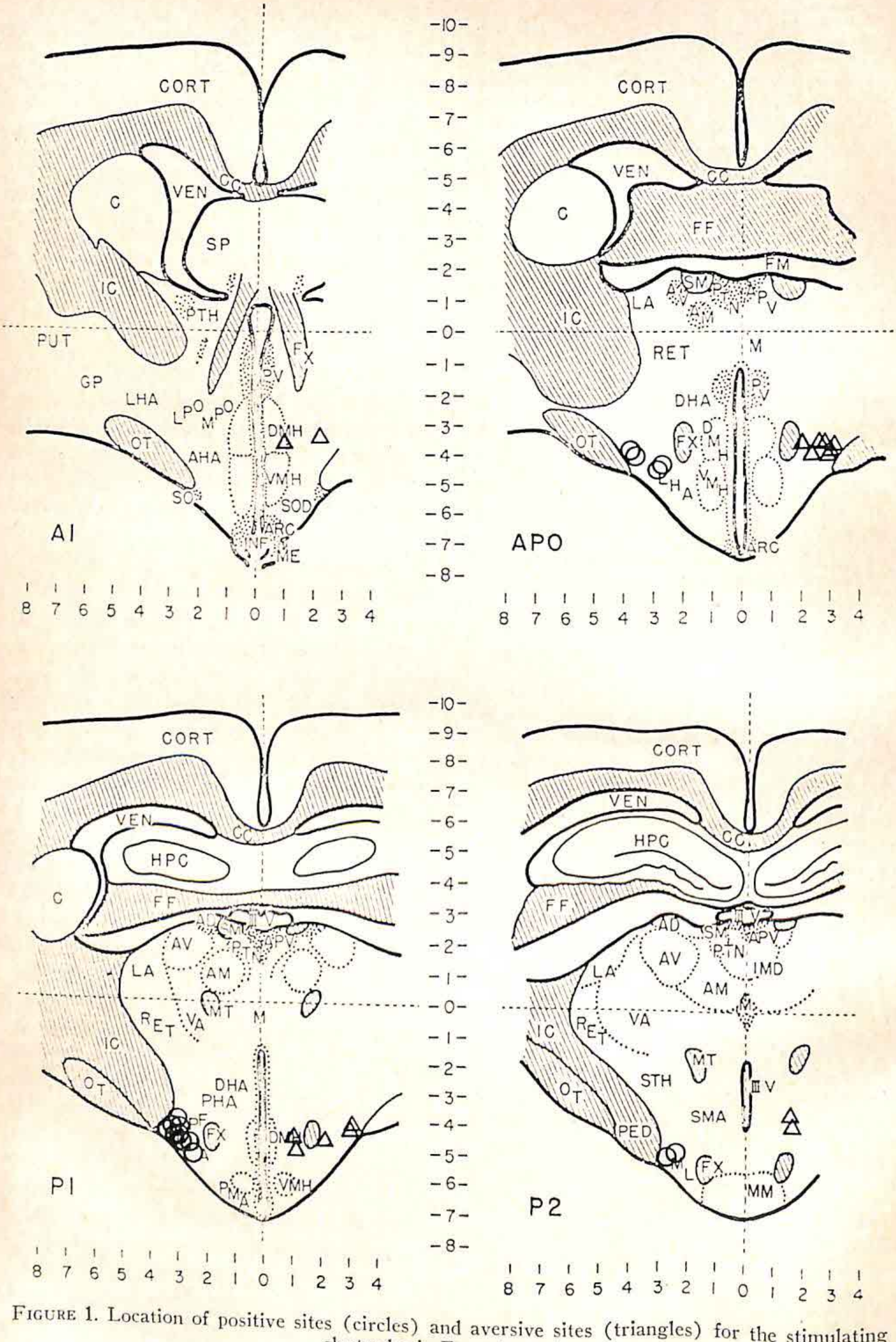

FIGURE 1. Location of positive sites (circles) and aversive sites (triangles) for the stimulating electrodes in Experiment 1.

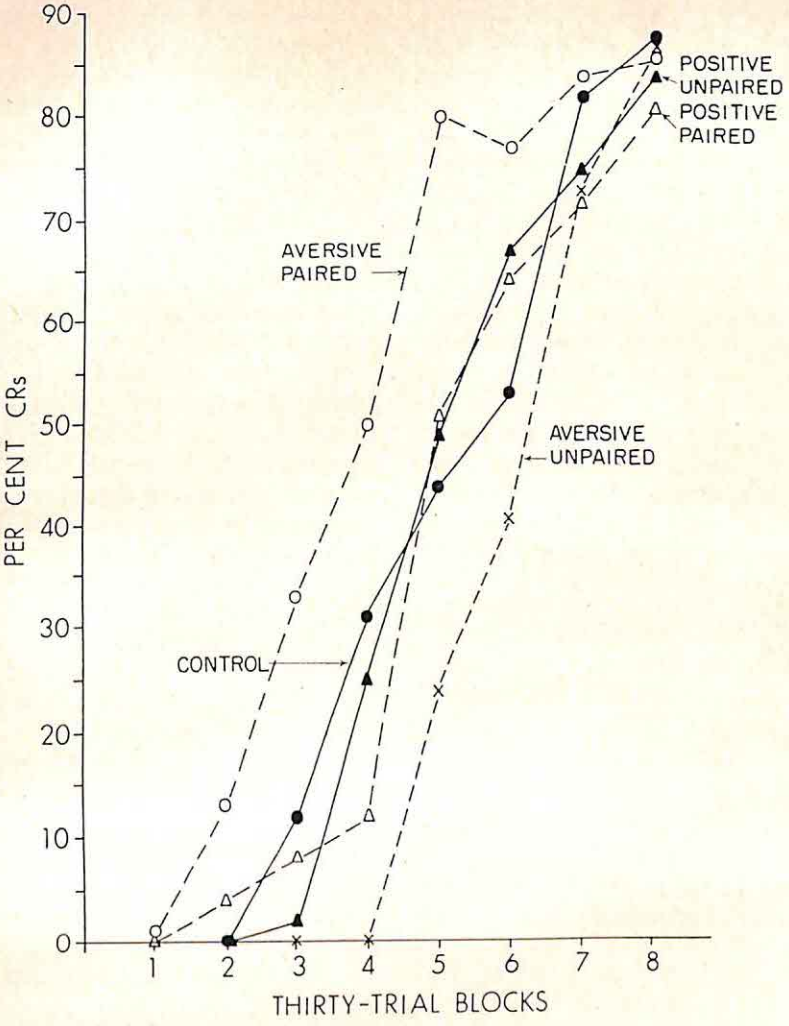

FIGURE 2. Frequency of conditional responses for the five experimental groups during the acquisition phase of Experiment 1.

experimental groups is depicted in Figure 2. The rate at which each animal conditioned was assessed by measuring the number of trials which each required before reaching a criterion of 10 CRs. The implanted controls ($n = 16$) reached criterion in 146.8 trials. The two groups with positive electrode placements reached criterion in 146.2 trials (ICS + US paired, $n = 8$) and 147.4 trials (ICS + US unpaired, $n = 8$). The two groups with aversive electrode placements reached the acquisition criterion in 94.3 trials (ICS + US paired, $n = 9$) and in 171.7 trials (ICS + US unpaired, $n = 7$). A one-way analysis of variance among these five groups indicated reliable between-group differences, $F(4, 43) = 3.89$, $p < .01$. A Newman-Keuls multiple-comparison test ($df = 43$) indicated that the aversive paired group acquired more rapidly than all other groups ($p < .02$) and that the aversive unpaired group was not significantly slower than the implanted controls or the two positive placement groups.

The terminal performance level for each animal was assessed by analyzing the percentage CRs on the last 30 trials of Day 4. The five experimental groups did not show any significant differences on this measure. The mean performance level for the implanted controls was 88.5%. This same measure was 87.8% for the aversive paired group, 86.7% for the aversive unpaired group, 81.1% for the positive paired group, and 83.8% for the positive unpaired group.

The differential conditioning performance of the five experimental groups on Days 5, 6, and 7 was analyzed in terms of overall hit rate (percentage CRs to CS+) and false alarm rate (percentage CRs to CS−). The hit rate and false alarm rate for each animal was converted into a discrimination index (SI) which equals 1.0 for perfect discrimination and .0 for nondifferential responding (Frey & Colliver, 1973). The formula for this measure is

$$\mathrm{SI} = \frac{\text{hit} - \text{false alarm}}{2\,(\text{hit} + \text{false alarm}) - (\text{hit} + \text{false alarm})^2}.$$

The 16 implanted control animals had an average discrimination index of .14. This reflected a mean hit rate of 82% and a mean false alarm rate of 73%. These values indicate that the control rabbits evidenced little discrimination between the 1,200-Hz and 1,000-Hz tones. The animals that received stimulation at either positive or aversive sites generally showed better discrimination performance and more clearly so in the paired groups. The positive-paired group had a hit rate of 85%, a false alarm rate of 47%, and a discrimination index of .42. The positive unpaired group had rates of 78% and 62% and an index of .19. The comparable data for the aversive site groups were 81%, 31%, and .55 for the paired group and 78%, 59%, and .20 for the unpaired group. Although the five groups had similar hit rates indicating similar levels of general responsivity, they differed reliably on the discrimination index, $F(4, 43) = 7.56$, $p < .001$.

A Newman-Keuls multiple-comparison test comparing the mean discrimination indices of individual groups indicated that the aversive paired group ($n = 9$) displayed better discrimination performance than the aversive-unpaired group ($n = 7$), $t(43) = 3.40$, $p < .01$, and the control group ($n = 16$), $t(43) = 4.88$, $p < .01$. The positive paired group ($n = 8$) displayed better discrimination performance than the positive unpaired group ($n = 8$), $t(43) = 2.27$, $p < .05$, and the control group, $t(43) = 3.22$, $p < .01$. The two unpaired groups were not reliably different from each other or from the control group. The two paired groups were not reliably different from each other.

The eyelid conditioning data were also examined on the basis of histological classification and observational criteria. The self-stimulators (i.e., positive ICS subjects) were divided into two groups—one ($n = 7$) with electrode placements in the area P1, L3, D4.5 (Sawyer, Everett, & Green, 1954), which corresponds to Bruner's (1967) high self-stimulation site, and the other with placements elsewhere. The first group made their 10th CR in an average of 121.4 trials, while the latter groups required 166.6 trials, $t(14) = 2.52$, $p < .03$. This difference was similar for both paired and unpaired groups. This result, in conjunction with the previous data, suggests that the rate of acquisition with positive ICS depends more on the electrode placement site than on whether the ICS is paired or unpaired with the peripheral US. Considering the post hoc definition of group membership for this analysis, however, this conclusion should be interpreted with caution.

Some animals with aversive electrode placements were initially classified as "highly aversive." This group differed from the remaining aversive animals in that they reacted more violently to the stimulation such that multiple tests were avoided for fear that the animals might injure themselves. Although there was no difference in acquisition rate for these two groups, they did differ in terms of terminal performance level. The highly aversive group averaged 91.7% CRs, while the remaining aversive group averaged 80.0% CRs, $t(14) = 3.26$, $p < .01$. Both paired and unpaired groups followed this pattern. The highly aversive paired group ($n = 6$) had an average discrimination index of .64, while the aversive paired group ($n = 3$) had a discrimination index of .36. The highly aversive unpaired group ($n = 4$) had an index of .17, whereas the aversive unpaired group ($n = 3$) had an index of .25. These results indicate that the effects of the aversive ICS seem to be enhanced as its aversiveness is increased.

Discussion

The main findings of the first experiment were that (a) aversive ICS paired with a peripheral US leads to more rapid acquisition and to better discrimination performance than that which is observed in nonstimulated, implanted controls or in animals receiving aversive ICS during the intertrial interval; (b) these beneficial effects of the paired aversive brain stimulation seem to be accentuated as the aversiveness of the brain stimulation is increased; (c) positive ICS (i.e., a self-stimulation site) paired with a peripheral US does not facilitate the rate of acquisition of the eyeblink CR as compared with nonstimulated, implanted controls or to animals receiving positive ICS during the intertrial interval; (d) positive ICS paired with the US does improve discrimination performance as compared with the above controls; and (e) positive ICS in the posterolateral hypothalamus, whether it is paired or unpaired with the cheek-shock US, may produce faster acquisition than that observed with stimulation in other positive ICS sites.

The present results with aversive brain stimulation are completely consistent with the data reported by Bruner (1966) and extend his findings by demonstrating that the facilitative effect applies to differential conditioning and is absent if the aversive brain stimulation is not paired with the peripheral US. The present observation that this facilitation is enhanced when the aversiveness of the ICS is greatest suggests that the added aversiveness per se may be sufficient to explain this facilitative effect. This interpretation is both plausible and parsimonious and is perfectly consistent with the well-established finding that increases in US intensity facilitate acquisition of the eyeblink CR (e.g., Smith, 1968).

The present findings with the positive brain stimulation are less consistent with Bruner's (1966) results. Pairing positive brain stimulation with the peripheral US in the present study did produce better discrimination performance but did not produce a general facilitation of CR acquisition. It is not clear why we failed to replicate his acquisition result. It is interesting, however, that those positive animals whose electrode sites most closely approximated his positive sites (Bruner, 1967) seemed to acquire the CR more rapidly than the other positive animals. This subgroup did not acquire significantly faster than the implanted controls, but our means (121 trials vs. 147 trials) were in the same direction as Bruner's. If a facilitative effect in acquisition exists, it is clear from our data that it is independent of whether the positive ICS is paired or unpaired with the peripheral US. This suggests that aversive ICS and positive ICS do not act the same way in acquisition. The effect of the aversive ICS depends on its pairing with the US and on its relative aversiveness. The facilitative effect of the positive ICS, if it exists at all, is not dependent on being paired with the US and seems to be specific to sites in the posterolateral hypothalamus.

EXPERIMENT 2

Bruner (1965) reported that in rabbit eyelid conditioning a bright light flash produced a vigorous UR but was ineffective in developing an overt CR. The purpose of our second study was to provide a further investigation of the light flash as a Pavlovian US. By combining a light flash with positive or aversive intracranial stimulation, we attempted to determine if Bruner's (1965) finding with the light-flash US could be altered by manipulating its affective characteristics.

Method

Subjects. The subjects were 59 male Dutch rabbits, 1.5–2.0 kg, procured from Scientific Small Animals, Inc., Arlington Heights, Illinois. The animals were housed as described in Experiment 1.

Apparatus. The apparatus, a potentiometer-polygraph recording system, and the method of restraint were the same as those employed in Experiment 1. One to four subjects were run simultaneously in the walk-in cooler. Adjacent to each Plexiglas restraining device was a light-flash unit. Bruner (1965) commented that light flashes may be ineffective because they have been used at too low an intensity. To avoid this problem, a high-intensity flash unit (Amglo Corp., HD-4X) delivering 35 mJ was used. This ensured that a reliable UR (150–200-msec latency) was elicited by the light-flash US. The CS was a 1,000-Hz tone at 80-db. SPL imposed on a constant white noise background of 70 db. The cheek shock and ICS were identical to those employed in the first experiment.

Procedure. All 59 animals were surgically prepared by embedding two bolts into the top of the skull and attaching two cheek electrodes beneath the right eye. Forty-four of these animals were also stereotaxically implanted with a chrome–nickel bipolar electrode directed at the postereolateral hypothalamus. At least 4 days after surgery each subject was tested in the self-stimulation box as described earlier. Approximately one third of the animals were self-stimulators, one third showed aversive reactions, and one third showed neither positive nor aversive reactions.

Before conditioning, each animal was given a 75-min. adaptation session in the stocklike restrainer to become habituated to the conditioning chamber. Following this the animals received 10 sessions (1 session per day) of conditioning trials in three stages. In each session they were given 50 trials with an average intertrial interval of 90 sec.

Stage 1, the pretreatment stage, consisted of three 75-min. sessions per day of the appropriate pretreatment. The control group (five implanted and six nonimplanted animals) received neither the CS, the light flash, nor the ICS. The CS-alone group (six implanted and five nonimplanted animals) received 50 tone presentations during each session. The flash group (four implanted and six nonimplanted animals) received 50 tone presentations paired with the light flash during each session. The implanted animals in each of the control, CS-alone, and flash groups received no brain stimulation. The ICS+ group ($n = 7$) received 50 tone presentations paired with the .5-sec positive ICS during each session, whereas the ICS− group ($n = 7$) received the same number of presentations of the tone followed by the .5-sec aversive ICS. The flash/ICS+ group ($n = 6$) received 50 tone presentations followed by the simultaneous onset of the positive ICS and the light flash in each session. The flash/ICS− group ($n = 7$) received the same number of tones followed by the simultaneous onset of the aversive ICS and the light flash. The CS–US interval for all groups was 500 msec.

After three days of pretreatment, all animals received 5 days of a transfer test in which the tone CS was followed by a cheek-shock US. Several animals which failed to reach asymptote within 5 days were given one extra session.

Results

Histology. The sites of the electrode tips for the animals that displayed self-stimulation rates of 15 bar presses per minute or higher are depicted as circles in Figure 3 in a manner identical to Figure 1. Eight of the 13 sites were located in the lateral hypothalamus in an area which corresponds closely to the major self-stimulation area depicted by Bruner (1967). The single positive site at P2 also corresponds to a self-stimulation area reported by Bruner. The remaining positive sites at APO do not correspond closely to Bruner's mapping. The aversive sites are depicted as triangles in Figure 3. Eight of the 14 sites located at APO correspond closely with aversive areas depicted by Bruner. The remaining 6 aversive sites at P1 are located in the same area that was highly positive for 8 animals in this study and 14 animals reported by Bruner.

Conditioning. A CR was defined the same as before except that the recording interval was restricted between 120 and 520 msec after CS onset. The control and CS-alone groups made no eyeblinks during the first stage. The groups receiving ICS displayed no CRs, but some eyelid activity was noted in response to the onset of aversive ICS. The groups receiving the light flash displayed a large, consistent eyeblink to the light flash with UR latencies of about 150–200 msec. Although this stimulus regularly followed the CS for 150 trials, no more than 5 CRs were made by any of these animals during the pretreatment stage.

The rate of CR acquisition for each of the seven experimental groups during Stage 2 is depicted in Figure 4. Trial-by-trial response protocols were made for each animal indicating whether a CR occurred on each of the 250 trials. This response sequence was examined using a computer program which derived the best fit parameters for a mathematical model described by Frey and Butler (1973). The model assumes that an all-or-none change in performance occurs on a single trial, K, and that the response probability after K is stationary at an asymptotic performance level. Trial K is analogous to

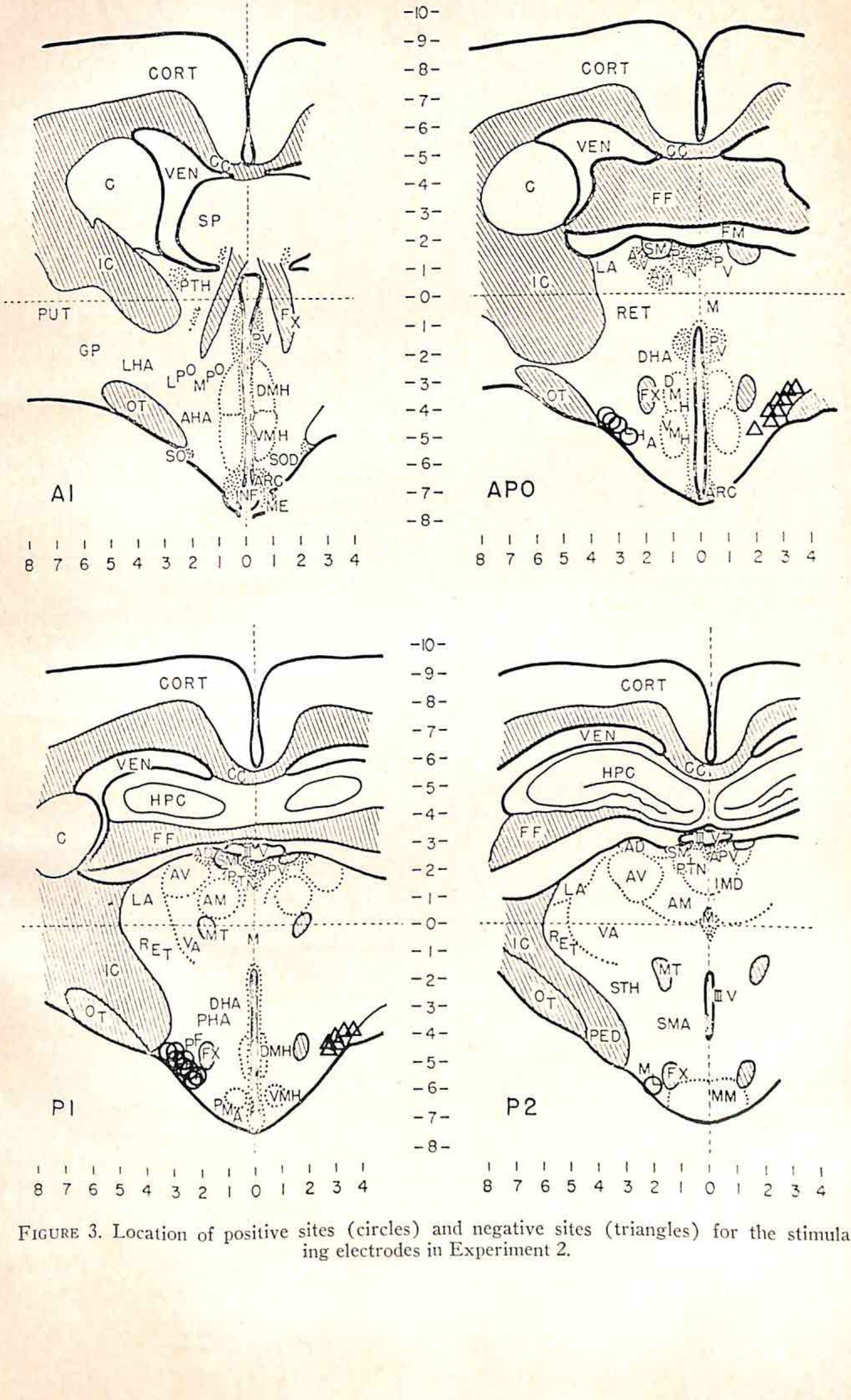

FIGURE 3. Location of positive sites (circles) and negative sites (triangles) for the stimulating electrodes in Experiment 2.

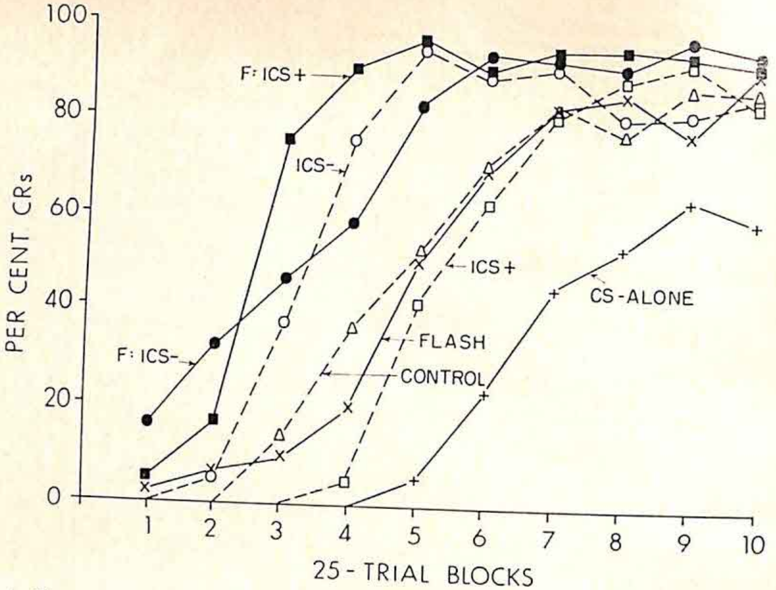

FIGURE 4. Frequency of conditioned responses (CRs) for the seven experimental groups in Experiment 2 during the conditioning phase (Stage 2). (F = flash; ICS = intracranial stimulation.)

other common measures of learning rate (e.g., trial of the 10th CR).

The mean values of K for the implanted animals in the control, CS-alone, and flash groups were 120, 192, and 95, respectively, as compared with 97, 167, and 118 for the nonimplanted animals in these same groups. An analysis of variance (ANOVA) showed no reliable main effects due to implantation ($F < 1$) or due to the Pretreatment × Implantation interaction ($F < 1$). Pretreatment had a highly reliable effect, $F(2, 29) = 8.32$, $p < .005$. On the basis of these results, the data from the implanted animals were combined with the data of the nonimplanted animals in their respective groups for subsequent analyses.

To assess the differential effects of the several pretreatment conditions, the transition trial, K, was examined for each group. An ANOVA on the group means indicated a reliable difference, $F(6, 52) = 10.57$, $p < .001$. Newman-Keuls multiple-comparison tests revealed that the seven groups in the second stage can be categorized into three sets ($p < .05$). The group means of flash/ICS+ (55), ICS− (67), and flash/ICS− (60) are not significantly different from one another and constitute the set with the lowest Trial-K values. The group means of control (108), flash (109), and ICS+ (120) are not significantly different from one another and constitute the set with intermediate Trial-K scores. The CS-alone group mean (181) constitutes the third set and has the highest Trial-K mean.

Discussion

When the tonal CS was paired with either the light flash or ICS or both during the initial phase of this study, there was no evidence of eyelid conditioning to the tone. The effect of each of the pretreatments became apparent only when the CS was subsequently paired wtih a conventional cheek-shock US. This transfer test indicated that (a) preexposure of the CS by itself prior to CS–shock pairings retarded the rate of CR acquisition (i.e., a "latent inhibition" effect); (b) prepairing the CS with the light flash prior to pairing wtih shock did not facilitate the rate of CR acquisition; (c) prepairing the CS with ICS prior to pairing with shock facilitated CR acquisition when the ICS was aversive but not when it was rewarding; and

(d) preparing the CS with a light-flash/ICS compound facilitated the subsequent rate of acquisition for both rewarding and aversive ICS.

The failure of the light flash to act by itself as an effective US is consistent with previous data reported by Bruner (1965) and is inconsistent with contiguity theory. Our failure to observe conditioning in Phase 1 with the compound US groups (light flash + ICS) indicates that adding an affective component to a reflex elicitor does not ensure that overt CRs will be elicited. Apparently, either the combination of reflex elicitation and affect is not sufficient or the particular combination we employed does not provide an appropriate "gestalt."

The transfer effects observed in Phase 2 indicate that the pairings of the CS with aversive ICS or with the light-flash/ICS compounds did have some impact. Whether the change in CS properties was related to associative factors, to some nonspecific attentional alteration, or to a combination of both cannot be ascertained from our data. The facilitation which was observed was relatively large in magnitude and, therefore, from at least an empirical standpoint, was not a trivial effect. The parallel facilitation produced by both the positive and aversive ICS when compounded with the light flash argues against any simple interpretation in terms of emotional conditioning to the CS.

The retarding effect of CS preexposures has been reported previously in leg flexion conditioning (Lubow & Moore, 1959), in the CER (conditioned emotional response) paradigm (Carlton & Vogel, 1967), and in the rabbit eyelid paradigm (Siegel, 1969). Reiss and Wagner (1972) have presented evidence suggesting that the retarding effect of the preexposures is produced by an attentional deficit rather than true inhibition. In the present study, prior pairing of the CS with the light flash eliminated this retarding effect. The group receiving this pretreatment acquired the CR at the same rate as the control group which received no explicit stimuli during the pretreatment. The flash seemed to prevent habituation to the CS (the flash pretreatment led to faster acquisition than the CS-alone pretreatment) but did not produce positive transfer to the conventional conditioning procedure in phase two.

Experiment 3

Bruner's (1966) study and Experiment 1 of this study demonstrate that ICS can substantially alter conditioning performance when it is compounded with a conventional cheek-shock US. Experiment 2 indicated that a light-flash/ICS compound can produce transfer effects which are not observed when the light flash is used as a US by itself. In discussion of these findings, we have not considered the general possibility that the ICSs employed may somehow have been effective by rewarding or punishing, in a Thorndikian sense, any preceding eyeblinks. Experiment 3 was designed to comment on such potential routes of influence.

Method

Subjects. Twenty-four of the rabbits from Experiment 1 were employed. Eight had electrodes in positive sites, eight had electrodes in aversive sites, and eight were implanted controls. The animals were housed and fed as previously described.

Procedure. The third experiment used the same conditioning equipment as the previous studies. The CS, US, and ICS were identical to those employed in Experiment 1. After completion of differential conditioning (Days 5–7) described in Experiment 1, each rabbit was retested in the stimulation box for several days. Approximately 1 week after completion of Day 7, 16 animals with functional electrodes each received 7 sessions on a contingent ICS schedule. There was one session each day with 80 trials per session. The CS–US interval was 1,200 msec (as compared with 600 msec during differential conditioning) and the intertrial interval was a constant 50 sec. An electronic Schmitt-trigger device was designed to automatically detect a CR and was used to provide response-contingent modifications of the conditioning schedule.

On trials in which no CR occurred, all three groups received a cheek-shock US 1,200 msec after CS onset. On trials in which an anticipatory CR occurred, the two ICS groups received no cheek shock but did receive *immediate* brain stimulation. That is, the ICS occurred immediately after the anticipatory eyeblink and the cheek shock was omitted from its regularly scheduled time of appearance. For the implanted controls, the cheek-shock US was omitted whenever the rabbit made an anticipatory CR. CS duration was always 1,300 msec (i.e., it was not response terminated).

When each rabbit had completed seven sessions on this contingent schedule, it then received an eighth session under a pseudoextinction schedule.

The paradigm differed from true extinction in that the two ICS groups continued to receive brain stimulation contingent upon making a CR. After this single pseudoextinction session, each ICS animal was paired with another animal in his own group (i.e., aversives were paired with aversives and positives with positives) and each pair then received six sessions of contingent ICS presentation under a yoking procedure. During the first three sessions, Animal 1 was yoked to Animal 2. On the second three sessions, Animal 2 was yoked to Animal 1. During each 80-trial session, the US was omitted and the ½-sec ICS train was presented to both animals whenever the contingent rabbit made a CR. This type of yoking arrangement has been described in detail elsewhere (Moore & Gormezano, 1961).

Results

A conditional response was defined as a 3-mm pen movement instead of a 1-mm movement as in Experiments 1 and 2. This more conservative definition was necessitated to ensure reliable response detection by the electronic scoring unit. The performance of the three experimental groups during the initial seven sessions of the avoidance paradigm is depicted in Figure 5. At the end of differential conditioning (Experiment 1, Phase 2), the performance of all 24 animals was at or above the 80% level to CS+. The shift to a longer CS–US interval (600 to 1,200 msec), a partial reinforcement schedule (i.e., the US no longer occurred on every trial), and a new, more conservative CR definition combined to produce an immediate drop in performance. On the first five trials of the first session, the three groups demonstrated similar performance (control = 70% CRs, ICS+ = 75% CRs, ICS− = 78% CRs). However, on the last 75 trials of the first session, the control animals with 43% CRs responded reliably more often than the combined ICS groups (ICS+ = 24%, ICS− = 20%), $t(22) = 2.23$, $p < .05$. This abrupt decrement in performance was followed by a gradual increase in CR frequency which appeared to asymptote at approximately 60% CRs for all three groups. During the last three sessions of this phase, there were no reliable differences among the three groups.

During the pseudoextinction session, the aversive ICS group made an average of 14.1 CRs, the positive ICS group made 7.9 CRs, and the implanted controls made 10.8 CRs. An analysis of variance on the three groups, followed by a Neuman-Keuls test comparing

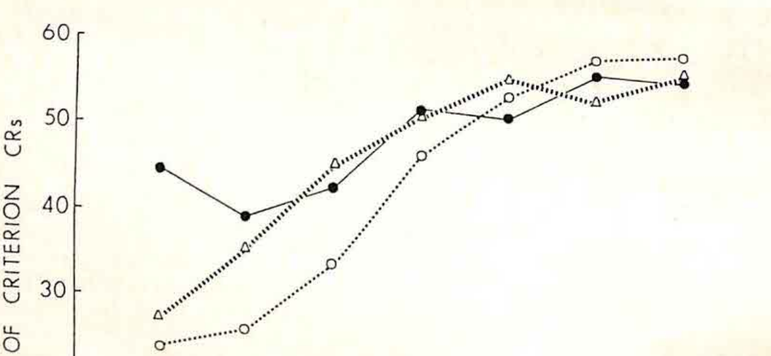

FIGURE 5. Frequency of conditioned responses (CRs) for the experimental groups during the first phase of Experiment 3. (ICS = intracranial stimulation.)

the means of the aversive ICS group and the positive ICS group, indicated a reliable difference, $t(21) = 2.36$, $p < .05$. Continuation of the contingent aversive ICS during extinction led to greater persistence than continuation of the contingent positive ICS.

The subsequent six sessions on the yoked avoidance schedule indicated that the noncontingent animal (i.e., the yoked rabbit) performed less well than his contingent partner independent of the polarity of the ICS. Since each animal served in both roles, the dependent measure was the number of CRs during the 3 contingent days minus the number of CRs during the 3 noncontingent days. The mean difference (contingent minus noncontingent) for the aversive ICS group ($n = 8$) was 63.1 CRs and for the positive ICS group ($n = 8$) was 85.0 CRs. All 16 animals showed a difference score in the positive direction so the superiority of the contingent procedure is highly reliable. The aversive ICS and positive ICS groups, however, were not reliably different from each other, $t(14) = 1.30$.

Discussion

The major findings of the third experiment were that (a) switching from a 100% Pavlovian reinforcement schedule to an avoidance schedule at a longer CS-US interval had an immediate, highly dramatic, decremental effect, and this decrease in performance was accentuated by CR-contingent ICS presentation; (b) adding CR-contingent presentation of aversive ICS or positive ICS to CR-contingent US omission (i.e., avoidance) did not improve or degrade asymptotic CR performance as compared with an avoidance contingency without ICS; (c) transfering from a CR-contingent US omission and ICS presentation schedule to a CR-contingent ICS only (i.e., pseudoextinction) led to greater persistence with aversive ICS than with positive ICS; and (d) CR-contingent US omission and ICS presentation produced much better CR performance than a noncontingent (i.e., yoked) procedure, and this effect was independent of the polarity of the ICS.

In combination these findings suggest that the CR-contingent positive ICS that was employed in this study did not act as a Thorndikian reward nor did the aversive ICS act as a Thorndikian punishment. The greater initial decrement produced by the ICS groups during the initial session of Phase 1 and the difference between the two ICS groups during pseudoextinction indicates that the animals were clearly aware of the contingent ICS. One would expect that a CR-contingent positive ICS would be much more rewarding than a CR-contingent aversive ICS. Therefore, the lack of any differential effects of the positive and aversive ICS at the end of the first phase strongly question a Thorndikian interpretation of the ICS effects in Experiments 1 and 2.

The yoking procedure used in the second phase of the third experiment led to very poor performance on the part of the yoked animals. Of the 16 animals, 4 had sessions as yoked controls in which they made fewer than 10 CRs, while their contingent partners made more than 60 CRs. These same 4 animals when they were on the contingent schedule averaged 55 or more CRs per session. One reason why the yoked animals made so few CRs is that the immediate presentation of the ICS, whenever the contingent animal made a CR, provided a reliable signal for the yoked rabbit indicating that the US would be omitted. ICS presentation therefore acted in the role of a conditioned inhibitor.

GENERAL DISCUSSION

The importance of the affective role of the US has been noted by previous authors. Bruner (1969) discusses "emotionally neutral" USs which reliably produce reflex motor effects but are ineffective in developing a CR. Sideroff, Schneiderman, and Powell (1971) reported that cardiac conditioning in the rabbit failed to occur with a low-intensity ICS even though a reliable UR was observed. They concluded that "mere S–S or S–R contiguity is insufficient to account for cardiac classical conditioning" (p. 8). Zamble (1974) demonstrated that pairing an auditory CS with positive ICS in curarized rats produced reliable activity changes to the CS when the animals were tested in the nondrugged state. Brain stimulation by itself seemed to be sufficient for producing associative effects even though no apparent motor response was involved.

The importance of the affective characteristics of the US has also been stressed by Colavita (1965) in discussing the dual role of the US, in which one component is the "reflex mechanism" whereby the US reliably evokes the UR and the other component is an "intensifying mechanism" that magnifies the emotional consequences of the CS–US pairing. On this same theme, Frey and Ross (1968) postulated that the elaboration of a CR requires the development of an associative connection between the CS and the motor pathways for the CR and the development of CS-elicited brainstem activation exerting a facilitative influence upon the motor pathways.

The present studies investigated the affective role of the US by employing brain stimulation in conjunction with more conventional conditioning stimuli. The results of the first experiment indicated that the combination of aversive ICS and a medium-intensity cheek shock is more effective for conditioning than the cheek shock alone. The data from the second experiment indicated that pairing a tone with aversive ICS or with an ICS/flash compound leads to positive transfer when the tone is later paired with a conventional US. These data provide additional evidence that the affective characteristics of the US are important in motor conditioning. In addition, our third experiment provided no evidence that ICS can act as a Thorndikian reinforcer for the eyelid response. Positive and aversive ICS seem to influence conditioning in a manner which is not related to a reward–punishment continuum.

At an empirical level, the present results permit a more detailed interpretation of previous work by Bruner. A light flash by itself, even when it is extremely bright and has a fast rise time, fails to produce CRs when it is employed as a US for eyelid conditioning. Combining the light flash with positive or aversive ICS does not change this outcome but does produce more subtle effects which show up as positive transfer to a conventional conditioning paradigm. The light flash by itself did not produce positive transfer. These outcomes in Experiment 2 are consistent with Bruner's (1965) original interpretation that the ineffectiveness of the flash is related to its affective neutrality.

Our results with the ICS/cheek-shock compound (Experiment 1) extend Bruner's (1966) findings. We replicated Bruner's finding that the combination of aversive ICS and cheek shock produces more rapid conditioning than cheek shock alone and, in addition, showed that this facilitation depends on the contiguous pairing of the ICS and cheek shock. A control group receiving aversive ICS during the intertrial interval indicated that this facilitation is not produced by an enhancement in the general arousal level of the organism. Our results with positive ICS question the robustness of Bruner's findings and suggest that this effect, if it exists at all, may be localized to a specific brain region and is independent of the explicit pairing of the ICS and peripheral US.

There are several reports that indicate that ICS has complex effects. Ball (1967) reported that stimulation of sites which support self-stimulation behavior produces inhibition of evoked potentials at the level of the trigeminal nucleus. Ball's finding is consistent with reports that positive ICS can attenuate the effects of peripheral aversive stimuli (e.g., Buckwalter, Gibson, Reid, & Porter, 1967; Cox & Valenstein, 1965; Geller, 1970; Merrill & Anderson, 1970). Other research, however, has indicated that positive ICS may actually facilitate acquisition in Sidman avoidance and in one-way or two-way shuttle avoidance (Carder, 1970; Margules & Stein, 1968; Stein, 1965). Carder (1970) proposes that positive ICS facilitates the performance of a well-learned response but interferes with the acquisition of a new response. In addition, Cantor (1971) has demonstrated that the rat's preference for positive ICS covaries with the predictability of this artificial stimulation. In terms of the experimental procedure used by Bruner (1966) and us, Cantor's finding implies that the positive affect elicited by the ICS may be enhanced as conditioning proceeds because the rabbits should be learning the predictive relationship between the CS and the ICS as the number of pairings increases. Late in training, when the CS is a well-established predictor of each occurrence, the ICS should be more positive than it was on the initial conditioning trials. This gradual change in the affective impact of the positive ICS during acquisition provides a powerful contaminating factor which obscures any simple interpretation of its effect.

It is probable that ICS as employed within a conditioning paradigm has diverse effects. It clearly produces an immediate change in affect. It may also elicit long-term changes in arousal level. In addition, ICS may interfere with the habituation of the attentional response to specific cues in the conditioning situation. Given such multiple effects, it seems necessary to interpret Bruner's results and our present results with considerable caution.

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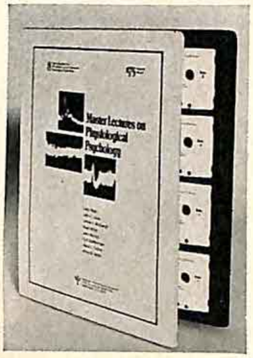

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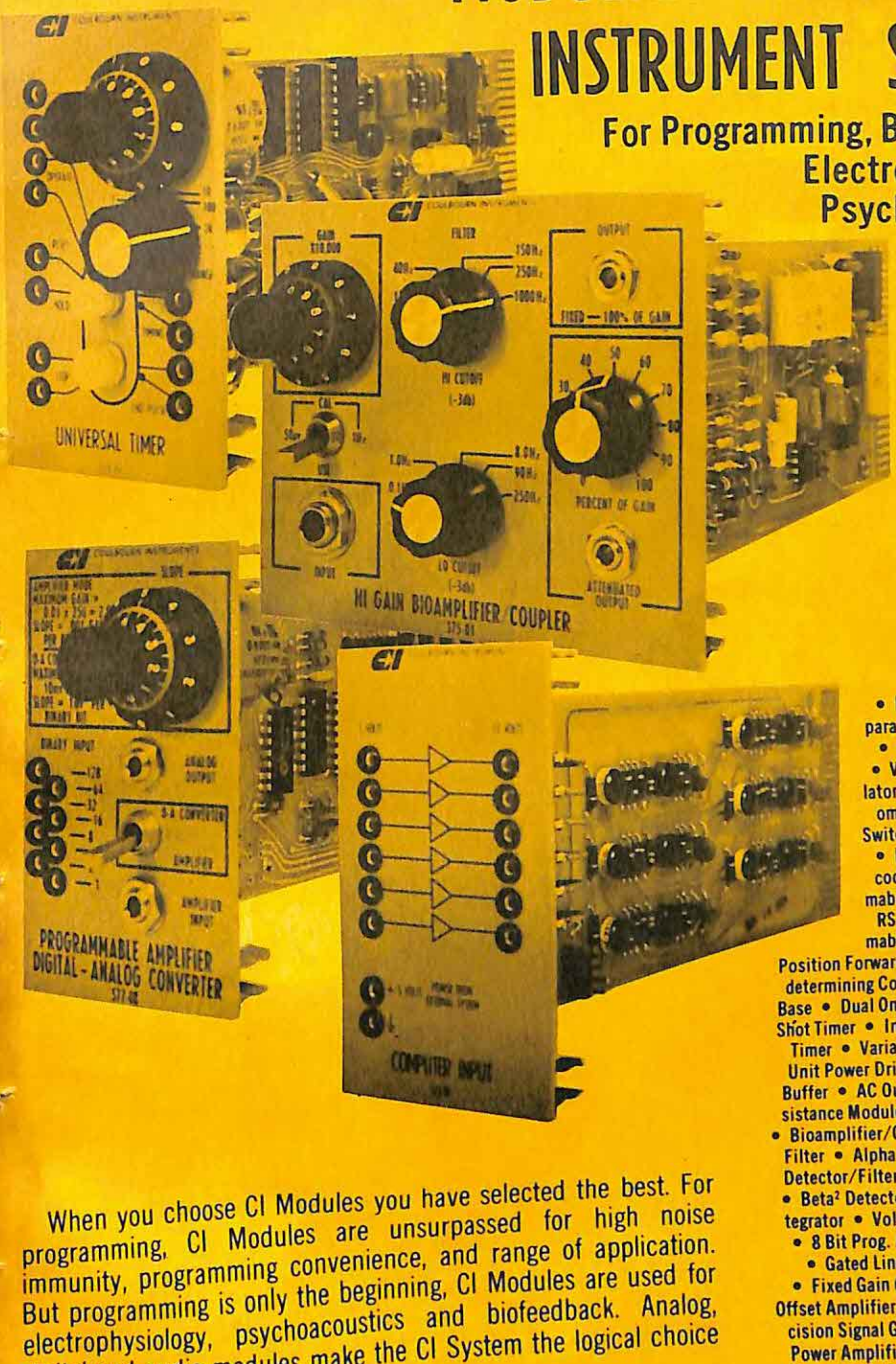

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Vol. 2, No. 3 July 1976

Long-Term and Transitory Interference Effects

Howard I. Glazer and Jay M. Weiss
The Rockefeller University

If animals receive inescapable electric shocks, their subsequent avoidance-escape learning is poor. This phenomenon, which can be called *the interference effect*, was studied in four experiments. Experiment 1 demonstrated that, depending on the parameters of the inescapable shock used, there exists a transitory effect and a separable, more permanent, long-term interference effect. Experiments 2 and 3 investigated the long-term effect, showing that it (a) required inescapable shocks of at least 5 sec duration in order to develop and (b) was still evident 1 week after such inescapable shock. It is suggested that, whereas the transient effect is produced by a short-lived neurochemical change, the long-term effect is mediated by a learned response. Consistent with this differentiation, Experiment 4 showed that the interference effect measured 30 min. after inescapable shock did not occur when animals had been repeatedly exposed to the type of inescapable shock that produced the transitory effect, whereas the interference effect measured 72 hr. after inescapable shock became more pronounced when animals had been repeatedly exposed to the type of inescapable shock that produced the long-term deficit. Aspects of the data suggest that learned helplessness is not the basis of the long-term interference phenomenon.

Deficits in avoidance-escape learning after uncontrollable, but not after controllable, electric shocks were reported by Overmier and Seligman (1967) and Seligman and Maier (1967) in dogs. To account for the interference with avoidance-escape behavior that followed inescapable shock (hereafter referred to as the *interference effect*), these investigators proposed that the animals had learned to be "helpless." According to this "learned helplessness" hypothesis, animals exposed to electric shocks that they could not control learned that their behavior did not alter their aversive environment. Having learned that shock termination was not contingent on their behavior, these animals, when exposed to the subsequent avoidance-escape training, (a) did not initiate attempts to terminate shock and (b) could not form appropriate associations when successful attempts were made, so that they therefore failed to acquire the avoidance-escape response. Animals initially exposed to electric shocks that they could terminate by performing a designated instrumental response did not learn to be helpless and were, therefore, able to acquire a subsequent avoidance-escape response.

Certain characteristics of the avoidance-escape deficit observed in the original dog studies, however, suggested that learned

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elplessness might not be an adequate explanation for the behavioral results. In the Overmier and Seligman (1967) experiment, for example, the poor avoidance-escape performance of the dogs was evident 24 hr. after inescapable shock but was absent if the dogs were first tested 48 hr. after shock. As noted by Weiss, Krieckhaus, and Conte (1968) and by Miller and Weiss (1969), this rapid dissipation of the avoidance-escape deficit is not characteristic of learning, since learned responses are generally more persistent (e.g., Miller, 1967; Skinner, 1950; Wendt, 1937). Another difficulty for the proposal that the interference effect arose from a learned response was the fact that stimulus similarities and differences between the situation in which inescapable shock was administered and the situation in which the test was given did not affect the magnitude of the avoidance-escape deficit or the percentage of subjects that showed the effect (Maier, Seligman, & Solomon, 1969, p. 321).

The foregoing facts suggested to us that the original avoidance-escape deficit observed after inescapable shock may not have been due to learning. In contrast to the learned helplessness hypothesis, we proposed (Weiss, Stone, & Harrell, 1970) that the poor avoidance-escape behavior observed in these dog studies was a performance deficit. Specifically, we suggested that the behavioral deficit following the severe inescapable shock used in these experiments was mediated by a temporary disturbance in central neurotransmitter activity that was produced by the intense inescapable shocks. This account was called the *motor activation deficit hypothesis* (Weiss, Glazer, & Pohorecky, 1976). The results from a series of behavioral (Weiss & Glazer, 1975), biochemical (Weiss, Glazer, Pohorecky, Brick, & Miller, 1975; Weiss et al. 1970), and pharmacological (Glazer, Weiss, Pohorecky, & Miller, 1975) investigations all supported the motor activation deficit hypothesis, providing evidence in a number of instances that contradicted a learned helplessness explanation.

On the other hand, although it can be argued that the motor activation deficit hypothesis provides a better account of the avoidance-escape deficit observed in the original dog studies, we have pointed out that it certainly will not explain all interference effects (Weiss & Glazer, 1975; Weiss et al., 1976). The motor activation deficit hypothesis attempts to explain the effects of certain intense stressors. Recently, Maier, Albin, and Testa (1973) have demonstrated an avoidance-escape deficit in rats after inescapable shock of moderate intensity, and Seligman, Rosellini, and Kozak (1975) have demonstrated that a similarly-produced deficit in rats lasts as long as 1 week after exposure to inescapable shock. Thus it appears that there is another type of interference effect in addition to that originally seen. The results to date indicate that whereas the temporary deficit produced by high-intensity inescapable shocks, such as seen in the original dog studies (Overmier & Seligman, 1967; Seligman & Maier, 1967) and our own previous work on rats (Weiss et al., 1976), is well-handled by the motor activation deficit hypothesis, the more recent work of Maier et al. (1973) and Seligman et al. (1975), showing long-term deficits after lower levels of inescapable shock, suggests the existence of an avoidance-escape deficit based on learning.

In the series of experiments that follow, we first attempted to differentiate clearly those conditions that give rise to the two different types of avoidance-escape deficits (transient, or motor activation deficit, vs. long term). We then went on to isolate an important variable (duration of inescapable shock) for producing the long-term deficit and to describe certain characteristics of this deficit, i.e, its durability over time and how it is affected if the animal is repeatedly given the inescapable shock. Finally, the article concludes by discussing the ramifications of these findings for both the motor activation deficit and the learned helplessness hypotheses.

EXPERIMENT 1

The purpose of Experiment 1 was to determine whether two distinctly different inescapable shock schedules would both produce a subsequent deficit in avoidance-escape behavior, with the deficit in one case being transitory and in the other case being more permanent. In earlier experiments (Weiss & Glazer, 1975) that studied the transient deficit, shocks of 2 sec duration and 4 mA intensity were used, with a total of 180 shocks delivered over the course of a 60-min. session. In the present study, animals in one condition (called HiShk) again received this treatment. In the Maier et al. (1973) study, 64 shocks of 5 sec duration and 1 mA intensity were delivered randomly with an average shock density of 1/min. (variable time, 1-min. schedule). Similar parameters were used in a second condition (called LoShk) of the present study, except that for such animals the shock was 6 sec in duration rather than 5 sec. Subjects pretreated with either of the two treatments just described were then tested in a shuttle avoidance-escape apparatus to determine the presence of either a short-term (tested 30 min. after pretreatment) or a long-term (tested 72 hr. after pretreatment) avoidance-escape deficit.

Method

Subjects. The subjects were 35 naive male albino rats from Holtzman Farms, weighing 200–225 g at the time of testing. Each animal was individually housed and maintained under conditions of ad-libitum food and water.

Apparatus. The apparatus for delivery of inescapable shock consisted of small Plexiglas restraining boxes (described and pictured in Weiss, 1971). Shock was delivered from a constant-current shock generator (5,000-V maximum output, 1-MΩ fixed series resistance). Avoidance-escape testing was carried out in two-way shuttle boxes. The shuttle boxes were constructed of ¼-in. (.64-cm) Plexiglas and measured 12 in. × 12 in. wide × 13-in. high (30.5 cm × 30.5 cm × 33 cm). The floor was made of ⅛-in. (.32-cm) diameter stainless-steel rods spaced ½ in. (1.3 cm) apart. The boxes were divided in half lengthwise by a 2-in. (5.1-cm) high Plexiglas barrier. Two stainless-steel rods were mounted on the top of the barrier ½ in. (1.3 cm) apart; these rods could be electrified to prevent the animals from sitting on the barrier. The floor on each half of the boxes was independently spring mounted so that pressure depressed the floor and activated a microswitch. The shock source for the shuttle apparatus delivered 2,000 V maximum (root mean square) through a series resistance of 340 Ω. The two shock leads were attached to the bars lying at the extreme ends of the apparatus. Between these leads, the grid-floor bars were connected in series by neon bulbs. Eleven neon bulbs were wired in series between the 12 grid-floor bars on each side of the apparatus, making a total of 22 neon bulbs through which the current passed. The two additional bars positioned at the top of the barrier were wired in parallel to two of the bars on the grid floor. Speakers were mounted in the center on the top of the boxes for the delivery of auditory stimuli. The shuttle boxes were housed in sound-attenuated chambers equipped with ventilating fans.

Procedure. The subjects were randomly divided into five groups of seven animals each. Four groups received shock. These animals were placed into the shock apparatus as described in Weiss (1971), and two shock electrodes (No. 2-56 × ⅛-in. (.32-cm) stainless-steel set screws) were taped to their tails. Two groups of animals received 180 inescapable electric shocks (delivered to the tail) of 2 sec duration, 4 mA intensity (peak current), on a fixed-time, 20-sec schedule (HiShk groups). The two remaining groups of animals received 60 inescapable electric shocks (delivered to the tail) of 6 sec duration, 1 mA intensity, on a variable-time, 1-min. schedule (LoShk groups). The fifth group remained untreated (NoShk). One group from each of the two different shock treatments was tested on a two-way shuttle avoidance task 30 min. after treatment (LoShk-30 min. and HiShk-30 min.). The remaining two groups of shocked animals were tested in the same task 72 hr. after treatment (LoShk-72 hr. and HiShk-72 hr.). All shocked rats were returned to their home cages in the interval between shock treatment and testing. A NoShk group received no inescapable shock but was simply tested on the two-way shuttle avoidance task.

The sequence of events in the shuttle avoidance test began with presentation of a 100-cps, 80-dB tone (the conditioned stimulus; CS). After 5 sec of tone alone, an electric shock was delivered and the tone continued. The shock in the shuttle apparatus was set up so that a meter reading of .3 mA was recorded in the circuit with no rat on the grid floor. If the animal jumped from one side of the box to the other during the tone but before shock initiation, the tone was terminated and a 1-min. intertrial interval (ITI) was initiated (avoidance response). If the animal jumped from one side of the box to the other after shock onset, both the shock and the tone were terminated and an ITI was initiated (escape response). If a shuttle (crossing) response had not occurred after 20 sec of shock, then shock and tone automatically terminated and an ITI was initiated. The entire test consisted of a total of 30 trials. The data collected were latencies to shuttle from one side of the box to the other as measured from the onset of the tone. If the animal failed to respond within 25 sec from tone onset, so that the trial was automatically terminated, a latency of 25 sec was scored on that trial.

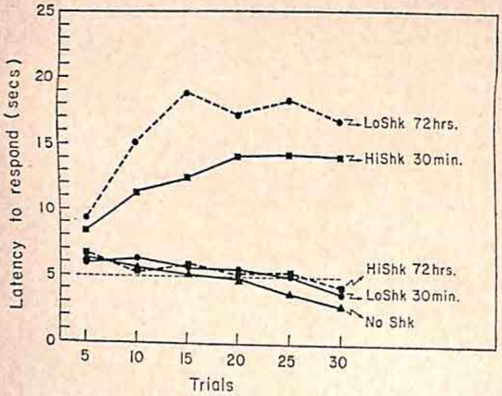

FIGURE 1. Mean latency to perform the correct response (barrier jump) in the shuttle task following onset of the warning signal (conditioned stimulus) for all groups. (Latency is plotted in blocks of five trials, with the last trial in each block indicated on the abscissa. Responses in less than 5 sec are avoidance responses; responses of longer latency are escapes. LoShk: groups were exposed to long-duration (6-sec), moderate-intensity (1-mA), inescapable shock before this test. HiShk: groups were exposed to high-intensity (4-mA) shock of brief duration (2 sec). Time designations (30-min., 72-hr.) indicate the time that elapsed between the inescapable shock and the shuttle test.)

Results and Discussion

Figure 1 shows the mean latency to perform the shuttle response plotted over blocks of five trials for each group, as measured from the onset of the CS. A Group × Trials analysis of variance indicated significant differences among the groups, $F(4, 30) = 12.07$, $p < .01$. Significant changes in response latencies over trials, $F(29, 870) = 1.91$, $p < .01$, and a significant Groups × Trials interaction, $F(116, 870) = 1.61$, $p < .01$, were also found. To clarify the group differences, Newman-Keuls pairwise comparisons were made. The results indicated that both the LoShk group tested 72 hr. after shock treatment (LoShk-72 hr.) and the HiShk group tested 30 min. after shock (HiShk-30 min.) differed significantly from the remaining groups ($p < .01$ in all cases) but not from each other.

The findings for the HiShk groups replicate the behavioral findings of Weiss and Glazer (1975). A deficit was seen when the animals were tested 30 min. after exposure to strong inescapable shock but was not seen when they were tested 72 hr. after the shock. Weiss and Glazer previously showed that this deficit following HiShk was gone by 48 hr. after the shock. The fact that the high-shock procedure reliably produced a deficit but one which was not present 48 or 72 hr. after inescapable shock supports the view that this deficit was not mediated by learning but by transitory stress-induced changes, possibly changes in the activity of central neurotransmitter systems as suggested by Weiss and Glazer (1975) and by Weiss et al. (1976).

The findings from the LoShk-72-hr. group support the existence of an avoidance-escape deficit based on learning. The presence of a deficit 72 hr. after LoShk indicates that a relatively long-term avoidance-escape deficit was produced by this LoShk procedure, as would be expected if the animals had learned something which interfered with subsequent performance. However, it is not clear why a similar avoidance-escape deficit was not evident 30 min. after treatment under LoShk conditions. It is possible that generalized arousal effects from low-intensity, inescapable shocks of the LoShk condition persisted for some time after this treatment, and that such effects can override a learned interference effect if testing occurs at a short (30-min.) interval after the LoShk treatment. In any event, it is quite clear that LoShk conditions give rise to a long-term avoidance-escape deficit whereas HiShk does not.

EXPERIMENT 2

In Experiment 1, the two inescapable shock treatments (HiShk and LoShk) differed in several parameters, including intensity, duration, and number of shocks, as well as duration of the intershock interval. Observations of the animals receiving LoShk in the first experiment suggested that a critical parameter in producing the long-term avoidance-escape deficit arising from this type of shock might have been the duration of each inescapable shock. In response to the onset of tail shocks, the animals struggled and thrashed about, vocalizing, urinating, and defecating. This initial reaction lasted for about 3 or 4 sec, giving way to a second phase of less active behavior that persisted until shock termination. As trials progressed, the second, inactive, phase began at an earlier time during the shock. In pilot work using short-duration (2 sec) shocks of similar intensity (1 mA), we also observed the active, initial reaction to tail shocks described above. However, under these conditions, the second stage of behavior (more passive reaction to continued shock) was not observed at the time when shock terminated (the animals were still very active) nor did this inactive phase emerge as trials progressed. Subjects treated in this manner did not show any subsequent deficit in avoidance-escape behavior. These observations suggested that the duration of each inescapable shock might have to be sufficently long for the animal to pass into the less active stage of behavior in order for the subsequent long-term avoidance-escape deficit to appear. Experiment 2 examined the effect of varying the duration of the inescapable shocks on subsequent avoidance-escape behavior in order to test this hypothesis.

Method

Subjects. The subjects were 35 naive rats of the same description as in Experiment 1.

Apparatus. The apparatus was the same as in Experiment 1.

Procedure. The subjects were divided into five groups of seven animals each. All subjects received inescapable electric shocks delivered to the tail as described in Experiment 1. Each group of animals received shocks of a different duration, either 2, 3, 4, 5, or 6 sec in duration. The interval between each shock was kept constant for all the groups, with shock delivered on a variable-time 1-min. schedule. To equate the total duration of shock for the groups, they received, respectively, 180 shocks (2-sec group), 120 shocks (3-sec group), 90 shocks (4-sec group), 72 shocks (5-sec group), and 60 shocks (6-sec group), so that all groups received a total of 360 sec of shock. Since the groups differed in the number of shocks received, as well as the duration of each shock, it is important to note that the groups receiving short durations of shock received the most shock trials. Since our prediction was that longer durations of shock would produce the avoidance-escape deficit but those of shorter duration would not, the greater number of shock trials given to short-duration groups was viewed as a cautious procedure, potentially working against the expected results.

Seventy-two hours after the administration of the inescapable shock treatment, all animals were tested in the two-way shuttle avoidance task described in Experiment 1.

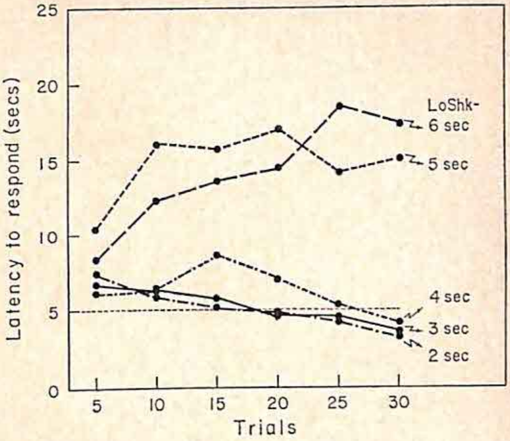

FIGURE 2. Mean latency to perform the correct response (barrier jump) in the shuttle task following onset of the warning signal (conditioned stimulus) for all groups. (Latency is plotted in blocks of five trials, with the last trial in each block indicated on the abscissa. Responses in less than 5 sec are avoidance responses; responses of longer latency are escapes. All groups received inescapable shocks of 1-mA intensity 72 hr. before this test. Time designations indicate the duration of the inescapable shock that each group received.)

Results and Discussion

Figure 2 shows the mean latencies to perform the shuttle for each group, plotted over blocks of five trials. A Group × Trials analysis of variance performed on the latency data indicated significant group differences, $F(4, 30) = 5.71$, $p < .01$. Pairwise comparisons showed that groups receiving either 5-sec or 6-sec inescapable shock did not differ from each other but did show significantly longer latencies than the remaining groups (Newman-Keuls, $p < .01$ in all cases), which in turn did not differ from each other. A significant Group × Trials interaction effect was also found, $F(116, 870) = 2.25$, $p < .01$, indicating that latencies increased over trials for the 5-sec and 6-sec shock groups, characteristic of the avoidance-escape deficit that we noted in Experiment 1, and decreased over trials for the remaining groups, characteristic of normal avoidance-escape learning.

The results of this experiment show that animals treated with inescapable shock of

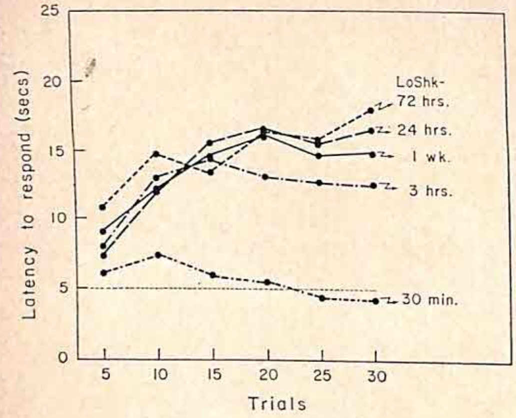

FIGURE 3. Mean latency to perform the correct response (barrier jump) in the shuttle task following onset of the warning signal (conditioned stimulus) for all groups. (Latency is plotted in blocks of five trials, with the last trial in each block indicated on the abscissa. Responses in less than 5 sec are avoidance responses; responses of longer latency are escapes. All groups received LoShk (shocks of 6-sec duration, 1-mA intensity) before this shuttle test. Time designations indicate the time that elapsed between the end of the inescapable shock and the shuttle test for each group.)

up to 4 sec appeared to perform like naive (NoShk) animals on the subsequent avoidance-escape task, whereas similar shocks of 5 or 6 sec duration produced a large deficit in avoidance-escape behavior. This finding should be considered in conjunction with our above-mentioned observations of animals' behavior during inescapable shock, which indicated that shock had to continue for several seconds before the animals entered the second stage of more passive behavior. It is also important to note that in the recent work of Maier and co-workers (Maier et al., 1973; Maier & Testa, 1975), the duration of the inescapable shock was at least 5 sec, and in the recent work of Seligman and co-workers (Seligman & Beagley, 1975), the shock duration was generally about 15 sec in duration. Thus, recent studies have used shocks of 5 sec or more in obtaining interference effects in rats.

EXPERIMENT 3

In Experiment 2, duration of the inescapable shock was shown to be an important variable in producing a long-term avoidance-escape deficit. The purpose of the present experiment was to evaluate further the development and persistence of the avoidance-escape deficit observed after long-duration, moderate-intensity, inescapable shock. The present experiment tested animals in the avoidance-escape task 30 min., 3 hr., 72 hr., or 1 week after a session of LoShk.

Method

Subjects. The subjects were 35 naive rats as described in Experiment 1.

Apparatus. The apparatus was the same as that described in Experiment 1.

Procedure. The subjects were divided into five groups of seven animals each. All received inescapable electric shocks delivered to the tail as described in Experiment 1. The shocks were 1 mA in intensity and 6 sec in duration and were delivered on a variable-time, 1-min. schedule. The shock session consisted of 60 shocks and lasted for 1 hr. Groups were tested 30 min., 3 hr., 24 hr., 72 hr., or 1 week after the inescapable shock session. The test was the avoidance-escape task in a two-way shuttle box, as described in Experiment 1.

Results and Discussion

Figure 3 shows the mean latencies to perform the shuttle response for each group, plotted over blocks of five trials. A Groups × Trials analysis of variance was applied to the data. The analysis yielded a significant main effect of groups, $F(4, 30) = 5.20$, $p < .01$, while Newman-Keuls pairwise comparisons indicated that the group tested after 30 min. differed from the group tested after 3 hr., which in turn differed from the remaining groups ($p < .01$ in each case). These remaining groups did not differ from each other.

The foregoing results do indeed show that the deficit in shuttle avoidance-escape responding that we observe after long-duration, moderate-intensity, inescapable shock (LoShk) is a long-term one, persisting for at least 1 week. Also, the results again showed, as in Experiment 1, the absence of the deficit 30 min. after the shock. This finding, together with the appearance of the avoidance-escape deficit 3 hr. after the shock, is consistent with the previously-mentioned possibility that LoShk produces, in addition to the learning that causes interference, some sort of transient excitatory residue that can override this learning, but that this residue disappears quite rapidly. There is some suggestion in Figure 3 that the interference effect increased in strength up to 72 hr. and thereafter showed a slight decline, but this is speculative since the 24-hr., 72 hr., and 1-week groups did not show statistically significant differences. Significant trials, $F(29, 870) = 1.75$, $p < .01$, and Groups × Trials, $F(116, 870) = 2.07$, $p < .01$, effects were also found. As seen in Figure 3, these effects indicated that the latencies decreased for the 30-min. group while increasing for the remaining groups.

EXPERIMENT 4

In an earlier experiment (Weiss et al., 1975), a critical test was run to determine whether the avoidance-escape deficit that followed inescapable shock (and also cold swim) was produced by stress-induced neurochemical changes or by transfer of a learned response. That experiment studied how multiple exposure to inescapable shock (and cold swim) affected subsequent avoidance-escape performance. If the avoidance-escape deficit following shock results from a learned response, such as learned helplessness, then repeated exposure to sessions of inescapable electric shock should reinforce this learning and increase, or at least maintain, the magnitude of the behavioral deficit. In contrast to this, if the avoidance-escape deficit were produced by transitory stress-induced neurochemical changes, then the deficit should become less severe with repeated exposure to shock since such neurochemical changes habituate with repeated exposure to stressors (e.g., Weiss et al., 1975; Zigmond & Harvey, 1970). To test this differential prediction, animals were given a pretreatment consisting of 14 sessions of inescapable shock (4 mA, 2 sec duration) before being exposed to a final session of shock followed by the test. Animals that received no pretreatment but just the usual single shock session showed both a behavioral deficit (interference effect) in the avoidance-escape test and a biochemical disturbance (lowered levels of brain norepinephrine), whereas animals exposed to the pretreatment before the final shock session and test showed no behavioral deficit on the test as well as indications that neurochemical systems thought to be involved had habituated to the stressor (no norepinephrine deficiency and decreased re-uptake). These data were interpreted as being supportive of a stress-neurochemical hypothesis for explaining the effects of high-intensity inescapable shock and contrary to the learned helplessness interpretation for such change.

The present experiment applied the same test to the avoidance-escape deficit produced by long-duration, moderate-intensity shock (LoShk). In Experiment 4, we again exposed some animals to a pretreatment consisting of daily exposure to the LoShk conditions and then compared the avoidance-escape performance of these animals with the performance of animals that had not received such pretreatment. If the deficit after LoShk was indeed produced by a learned response, then repeated exposure to the LoShk conditions would not be expected to ameliorate the deficit but might even make it worse since pretreated animals would simply have had repeated opportunity to learn the deleterious response. Thus, we expected repeated exposure to LoShk to have the opposite effect from repeated exposure to HiShk. To confirm that repeated exposure to HiShk would indeed ameliorate the deficit produced by HiShk, a HiShk pretreatment group was also included in the present study and compared with a no-pretreatment group that was given HiShk only once before the test.

Method

Subjects. The subjects were 28 rats similar to those described in the previous experiments and were maintained under the same conditions.

Apparatus. The apparatus was the same as that described in Experiment 1.

Procedure. The subjects were divided into four groups of seven animals each. All animals received inescapable electric shocks delivered to the tail as described in Experiment 1. One group (LoShk-No pretreatment) received a single 1-hr. session of shocks (1 mA intensity, 6 sec duration) delivered on a variable-time, 1-min. schedule, followed 72 hr. later by a shuttle avoidance-escape test. A second group (LoShk-pretreatment) received 10 sessions (1/day for 10 consecutive days) of the shock treat-

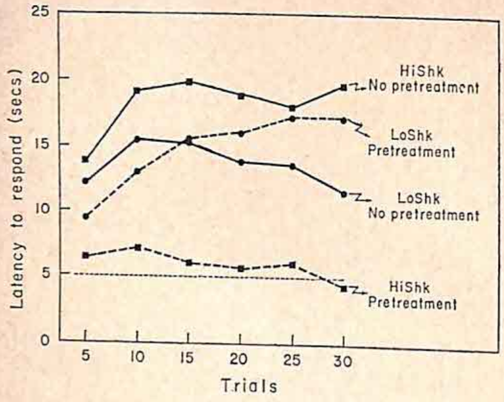

FIGURE 4. Mean latency to perform the correct response (barrier jump) in the shuttle task following onset of the warning signal (conditioned stimulus) for all groups. (Latency is plotted in blocks of five trials, with the last trial in each block indicated on the abscissa. Responses in less than 5 sec are avoidance responses; responses of longer latency are escapes. LoShk groups were exposed to long-duration (6 sec), moderate-intensity (1-mA), inescapable shock before this test. HiShk: groups were exposed to high-intensity (4-mA) shock of brief duration (2 sec) before this test. No pretreatment groups received the inescapable shock once before this test. Pretreatment groups received the inescapable shock 10 times (i.e., one session per day for 10 days) before the test. LoShk groups were tested 72 hr. after inescapable shock (or 72 hr. after the last session of shock for the pretreatment group): HiShk groups were tested 30 min. after inescapable shock [or 30 min. after the last session of shock for the pretreatment group].)

ment just described, with the shuttle avoidance-escape test given 72 hr. after the last shock session. A third group of animals (HiShk-No pretreatment) received a single 1-hr. session of shocks (4 mA intensity, 2 sec duration) delivered on a fixed-time, 20-sec schedule, followed 30 min. later by a shuttle avoidance-escape test. The fourth group of subjects (HiShk-pretreatment) received 10 session (1/day for 10 days) of this second type of shock with the avoidance-escape test given 30 min. after the last shock session. Thus, the first two groups of animals were exposed to the LoShk conditions (either pretreated or not pretreated) and then tested for the long-term avoidance-escape deficit that LoShk normally produces, while the second two groups were exposed to the HiShk conditions (again either pretreated or not pretreated) and then were tested for the short-term motor activation deficit that HiShk normally produces. The procedure for the shuttle avoidance-escape test was as described in Experiment 1.

Results and Discussion

Figure 4 shows the results of the shuttle avoidance-escape test for all four groups, with latency to respond plotted over blocks of five trials. The data were subjected to an analysis of variance using type of shock (4 mA, 2 sec vs. 1 mA, 6 sec), type of exposure (pretreatment vs. no pretreatment), and trials as factors. The analysis of variance yielded significant effects of type of exposure, $F(1, 24) = 5.18$, $p < .05$, and a Type of Shock × Exposure interaction, $F(1, 24) = 7.94$, $p < .01$.

For the HiShk animals, those that received the pretreatment (multiple shock exposure) did much better than those receiving only a single shock session prior to testing. These groups replicated our earlier work on the motor activation deficit (Weiss et al., 1975). For animals given LoShk, on the other hand, those that received the pretreatment did somewhat more poorly than those receiving only a single session of LoShk, although the difference was not statistically significant. Pairwise comparison tests showed that the HiShk pretreatment group showed significantly shorter latencies than did the remaining groups (Newman-Keuls, $p < .01$), which did not differ significantly from each other. Summarizing these results, when the parameters of training and testing were chosen to produce a motor activation deficit (HiShk condition; avoidance-escape testing 30 min. after last shock session), animals that received multiple exposure to shock showed much less of a deficit in the subsequent shuttle avoidance-escape task than did animals that received only a single session of shock. However, when the parameters of training and testing were chosen to produce a subsequent avoidance-escape deficit thought to be based on transfer of a deleterious learned response (LoShk condition; avoidance-escape testing 72 hr. after last shock session) the subjects that received multiple exposure to shocks performed at least as poorly as those that received a single exposure to inescapable shock.

A point worth noting is that with the LoShk treatment the multiple-exposure group showed only slightly longer latencies overall (14.9 sec vs. 13.6 sec average group latencies) than the single-exposure group. This suggests that most of the learning that occurred during inescapable shock and was responsible for the subsequent avoidance-escape deficit was accomplished in the first shock session.

General Discussion

The experiments in this series have demonstrated the existence of both transient and permanent interference effects following the administration of inescapable electric shocks. Experiment 1 demonstrated both of these effects. A single session of brief (2 sec), high-intensity (4 mA), inescapable shocks produced a short-lived deficit in subsequent shuttle avoidance-escape learning. As shown elsewhere (Weiss et al., 1976), this short-lived deficit appears to be based on a temporary neurochemical disturbance. It has been proposed that this short-lived deficit, based on stress-induced neurochemical changes, was what Seligman and co-workers observed in their dog studies (Weiss et al., 1970; Weiss et al., 1976) rather than learned helplessness as they proposed. The second type of interference shown in the present article followed the administration of long-duration (5 sec or more), moderate-intensity (1 mA), inescapable shocks. This treatment produced a long-term deficit, that is, a deficit that persisted for at least 1 week.

We presume that the long-term deficit (i.e., following long duration, moderate-intensity shock) observed in Experiment 1 was based on learning. The data supporting the presumption that the long-term interference effect was based on transfer of a deleterious learned response to the shuttle test comes from several sources. First, the long-term deficit was produced by shock of 1 mA intensity. As noted earlier, we have found (Weiss et al., 1975) substantial biochemical changes following administration of the intense (4 mA) electric shocks that produced the transient interference effect. However, no such disturbances have been observed when the lower intensity (1-mA) shocks have been used. Thus, it appears that, unlike the transient motor activation deficit-based interference, the participation of gross biochemical disturbances of the type shown previously as arising from more severe shock (Weiss et al., 1975) is unlikely.

As we have pointed out elsewhere (Weiss et al., 1976), one of the major requirements in demonstrating that a response is learned is to show that it persists over time. It was, in fact, the rapid dissipation over time of the interference effect in the dog experiments carried out by Seligman and co-workers that originally led us to suspect that learned helplessness was not an adequate account for the original effect. Experiment 3 in this article demonstrated that the interference effect following long-duration, moderate-intensity, inescapable electric shock was a long-term effect. Rats were tested in the shuttle avoidance-escape test at varying intervals up to 1 week after receiving inescapable shocks, and the interference effect was found to be present 1 week after such pretreatment with no significant decrease in its magnitude during the week.

A third source of support for the contention that the long-term interference effect was based on learning comes from Experiment 4. When strong (4-mA) inescapable shocks were used so that a single shock session produced the transient interference effect, then animals that received multiple sessions of this same type of shock showed no interference effect at the usual time that a deficit is observed (i.e., 30 min. after shock). These results replicated those of an earlier experiment and can be accounted for on the basis of neurochemical adaptation (Weiss et al., 1975). On the other hand, when moderate-intensity (1-mA) inescapable shocks were used so that a single shock session produced a long-term interference effect at the usual time of testing for this effect (i.e., 72 hr. after shock), then animals subjected to multiple sessions of this type of shock showed an interference effect that was somewhat (although not reliably) *larger* than the effect shown by animals receiving the single shock session. These results are taken as support for the contention that the long-term interference effect is based on learning, since one would expect a learned response to be reinforced by repetition of training conditions.

Incidentally, the results of Experiment 4 explained above are in agreement with the observation of Seligman (1975, pp. 70–71), who reported that repeated exposure to long-duration, moderate-intensity, inescapable shock would not reduce the avoidance-escape deficit. The results described in the present article make clear, however, that Seligman's finding occurred because of the type of inescapable shock used (i.e., LoShk), so that this finding did not bear on the results we had previously obtained with HiShk conditions and which we repeated in Experiment 4 of the present article.

Having delineated an interference effect based on a learned response, we now consider the important question of what was learned during the LoShk inescapable shock session that later interfered with avoidance-escape learning. If one suggests that the animals learned "helplessness," this hypothesis will explain most of the results found in the present article. However, some of the data found in Experiment 2 are not consistent with this hypothesis. In Experiment 2, animals received inescapable shocks of 2, 3, 4, 5, or 6 sec duration and then were tested 72 hr. later in the shuttle avoidance-escape test. The results showed that although *all* the foregoing groups were subjected to the conditions in which shock termination was not contingent on their behavior, only those that received shocks of *5 sec or longer* showed a subsequent interference effect. Since the learned helplessness hypothesis simply states that noncontingency is the basis of the interference effect, the failure to find the effect with noncontingent shock of less than 5 sec in duration is inexplicable by that hypothesis. Bracewell and Black (1974) have similarly noted a failure of certain conditions involving noncontingent shock to produce an interference effect and have pointed to a similar conclusion. They favor the view that animals learned competing motor responses that interfered with subsequent avoidance-escape performance. Our observations agree with the conclusion of Bracewell and Black. We also favor the view that animals learned competing behavior as a result of exposure to long-duration, moderate-intensity, inescapable shock.

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Long-Term Interference Effect: An Alternative to "Learned Helplessness"

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This article presents three experiments that explore whether inescapable shock of long duration and moderate intensity (LoShk) produces an avoidance-escape deficit (called an interference effect) by causing animals to learn to respond less actively or by causing them to learn to be "helpless." The first experiment showed that if animals given LoShk were subsequently tested in an avoidance-escape "nosing" response that required little motor activity, they learned and performed *better* than no-shock controls. The second experiment verified that the same LoShk treatment that led to this better performance in the first experiment would indeed interfere with subsequent avoidance-escape acquisition in those test situations that have previously been used to demonstrate the interference effect, that is, the three-response lever press, two-response shuttle, and single-response barrier jump. Finally, in the third experiment, using triplets in the classical avoidance-escape, yoked, and control animal paradigm, it was shown that yoked animals performed more poorly than either avoidance-escape or control animals on a subsequent three-response lever-press task but performed better on the nosing avoidance-escape task that required little movement. The results are interpreted as being compatible with the idea that animals acquire the tendency to be inactive as a result of exposure to long-duration, moderate-intensity, inescapable shock. The results are seen as incompatible with the idea that animals learn "helplessness," that is, not to associate behavior with its consequences, as a result of exposure to such shock.

In a preceding article (Glazer & Weiss, 1976), it was shown that what is called the "interference effect," that is, the disruption of avoidance-escape behavior by prior exposure to inescapable shock, occurs in at least two different ways. The first type of interference effect follows a single session of high-intensity, inescapable shock, and this deficit dissipates relatively rapidly over time. It is our contention that the original work done on the interference effect (Overmier & Seligman, 1967; Seligman & Maier, 1967), which used a single session of high shock and demonstrated a transient effect, is representative of this type of effect. Evidence strongly indicates that this short-lived effect is a performance deficit that seems to be based on transient changes in central neurotransmitter activity (Glazer, Weiss, Pohorecky, & Miller, 1975; Weiss & Glazer, 1975; Weiss, Glazer, Pohorecky, Brick, & Miller, 1975) and is not the result of any learned response such as learned helplessness.

The second type of interference effect follows the administration of long-duration (5 sec or more), moderate (1 mA) electric shocks and can be demonstrated at least a week after the inescapable shock treatment (Glazer & Weiss, 1976). Based on the modest intensity of the inescapable shock used and the persistence of the interference effect over time, this type of effect appears to be based on learning. The question now becomes: Is this second form of interference

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effect (that produced by long-duration inescapable shock) attributable to learned helplessness?

To evaluate this question, the meaning of learned helplessness should first be clearly defined. Learned helplessness is said to occur because, under conditions of inescapable shock, animals learn that there is no contingency between their behavioral responses and the alteration of their environment (see statements of theory in Maier, Seligman, & Solomon, 1969; Seligman, 1975; Seligman, Maier, & Solomon, 1971). Learned helplessness is considered to be a cognitive set or expectation that the organism acquires (e.g., Maier et al., 1969, p. 237; Seligman, 1975, pp. 147–148). As a result of acquiring this cognition, animals subsequently (a) do not make responses attempting to alter their environment and (b) do not readily make the association between a reward, such as termination of an aversive event, and a correct response even if this contingency happens to occur. Both a and b lead to poor performance in various tasks. In summary, then, learned helplessness is basically the learning of noncontingency, an expectation or cognitive set that is acquired when an animal experiences uncontrollable environmental events.

The present article describes a series of experiments that test whether the interference effect produced by long-duration, inescapable shock is the result of such learned helplessness. In a previous article (Glazer & Weiss, 1976), some evidence emerged to suggest that helplessness was not the basis for this effect. In Experiment 2 of that article, it was found that, if animals experienced noncontingency through exposure to inescapable shock, this was not per se sufficient to produce interference with subsequent avoidance-escape behavior. The interference effect only became evident if animals were exposed to inescapable shocks that lasted *5 sec or more*. Thus, duration of shock, not simply noncontingency, is critical. It is important to note in this regard that all of the recent articles purporting to demonstrate learned helplessness in the rat have used shock durations of 5 sec or longer (Bracewell & Black, 1974; Elmes, Jarrard, & Swart, 1975; Maier, Albin, & Testa, 1973; Maier & Testa, 1975; Seligman & Beagley, 1975; Seligman, Rosellini, & Kozak, 1975). This finding, that duration of shock is important, suggests an alternative to learned helplessness.

When shocks long enough to produce the interference effect were given, we noted that there appeared to be a biphasic activity response in reaction to the shock. First, subjects responded to shock onset by a great deal of movement. However, as the shock continued, the animals entered a second phase in which they became less active and remained relatively quiescent until shock termination. This observation suggested that animals might be learning to lower their activity level as a result of their being inactive at the instant of shock termination. Since all of the responses that have been used in testing for the interference effect have required a considerable amount of movement for successful performance, any learned tendency to be inactive would hinder performance. The experiments in this article are therefore aimed not only at testing the learned helplessness hypothesis but also at comparing it with the hypothesis that "learned inactivity" is responsible for the second type of interference effect.

EXPERIMENT 1

If it could be shown that animals given long-duration, moderate-intensity, inescapable shock would learn and perform an avoidance-escape response more rapidly and effectively than no-shock control animals, this would provide strong evidence against the view that animals learn to be helpless as a result of inescapable shock. The reason for this is as follows: If animals given inescapable shock begin training having learned that their behavior is not related to reinforcing events in the environment (which is what the learned helplessness hypothesis states), then in that training they should certainly not be able to acquire an avoidance-escape response more rapidly and perform it more effectively than no-shock controls. Although, as pointed out by Maier et al.

(1973), some testing situations may be insensitive for revealing interference effects, the negative effects of learned helplessness in such situations are presumed to be masked or so rapidly overcome that no behavioral deficit is observed. Thus, one would find no difference between animals given inescapable shock and no-shock controls in such situations. However, the learned helplessness hypothesis seems hard-pressed to explain how an animal that is presumed to begin training with a deficiency in its ability to form associations can show *better* acquisition than an animal that is naive. In contrast to this, superior acquisition after inescapable shock is not contradictory to the view that long-duration, moderate-intensity, inescapable shock causes animals subsequently to show a lower activity level. According to this position, inescapable shock could lead to improved acquisition of a response, provided that a low level of activity facilitated acquisition of the response used in the test.

We therefore sought a specific response to determine whether animals given inescapable shock might learn better than controls. Clearly, if animals given LoShk learn to be less active than normal animals, the task to be used should be one that might be acquired better by an animal that was relatively inactive. An appropriate task indeed was available. In earlier experiments (Weiss & Glazer, 1975) that studied the avoidance-escape deficit produced by strong inescapable shock (motor activation deficit), we had employed a "low-activity" avoidance-escape response in which an animal inside a tube was simply required to push its nose through a hole at the front of the tube in order to avoid and/or escape a shock. This response was therefore chosen for the present study. In fact, our original results using this task had shown that animals given strong inescapable shock were somewhat, though not significantly, superior to no-shock controls in learning this task (see Figure 5, Weiss & Glazer, 1975). Pilot experiments carried out prior to the present studies revealed that this difference became clearer when some of the original shock parameters were slightly changed, so the present studies were conducted with these new shock parameters.

Method

Subjects. The subjects were 14 naive male albino rats from Holtzman Farms, weighing 200–225 g. Each animal was individually housed and maintained under conditions of ad-libitum food and water.

Apparatus. The apparatus for the administration of inescapable electric shocks consisted of Plexiglas boxes (see Weiss, 1971, for description) and a constant-current shock generator (5,000 V maximum output, 1-MΩ fixed-series resistance) for the delivery of tail shock. The test apparatus was a "nosing" apparatus described in Weiss and Glazer (1975), which is similar to that shown in Weiss (1968). It consisted of a wire-mesh (hardware cloth) tube having a conically shaped end with a small hole (.75-in. [1.9-cm] diameter) at its center through which the animal could poke its nose to avoid and/or escape shock. The tube had a diameter of $2\frac{3}{8}$ in. (6 cm). A relay that responded to a capacitance field change produced by introduction of an object (rat's nose) into its field detected the nosing response. A small door covered the hole during the intertrial intervals, thereby preventing nosing responses except during trials.

Procedure. Subjects were randomly divided into two groups of seven animals each. One group received 60 inescapable shocks of 6 sec duration, 1 mA intensity on a variable-time, 1-min. schedule (LoShk). The description of how animals were placed in the inescapable shock apparatus and the affixing of electrodes can be found in Weiss (1971). This group was then tested in the nosing avoidance-escape task 72 hr. after this treatment. The second group (NoShk) received no inescapable shock treatment but simply was given the same nosing avoidance-escape test. The nosing avoidance-escape test was carried out as follows: A lightweight ring was slipped onto the animal's tail until it rested 2.5 in. (6.3 cm) from the base of the tail and then a short (.5-in. [1.5 cm]) piece of rubber tubing was taped to the animal's tail just behind the ring to prevent the ring from coming off the tail. The animal was then allowed to walk into a hardware cloth tube, and two metal bars were placed across the rear of the tube behind the animal 6.5 in. (16.5 cm) from the front of the tube. Since the bars prevented the animal from backing out of the tube and the lightweight ring prevented the animal from pulling its tail into the tube to turn around, the animal was therefore prevented from leaving the tube or turning around throughout the procedure. Two shock electrodes (No. 2-56 × $\frac{1}{8}$ in. [.32 cm] set screws) were quickly taped to the animal's tail behind the ring, and the first trial was then begun 60 sec later.

A trial was initiated by presentation of a 1,000-cps, 80-dB tone (conditioned stimulus; CS) together with the retraction of the door resting against the nosing hole, thereby opening the hole at the onset of the trial. Five sec after tone onset, a .75-mA tail shock was delivered to the shock electrodes. If the animal poked its nose through the hole at the front of the tube (extension of .25 in. [.64 cm] required) during the tone but before tone onset, the CS terminated (avoidance response) and the 1-min. intertrial interval (ITI) was initiated. If the animal performed the same response after the onset of the shock, both tone and shock terminated (escape response) and the ITI was initiated. If no response occurred after 20 sec of shock, the shock and tone automatically terminated and the ITI was initiated. Termination of a trial immediately caused extension of the door to recover the hole, thus precluding nosing responses during the ITI. The test consisted of 30 trials. Time to respond from the onset of the CS was measured. If the animal failed to respond within 25 sec so that the trial was automatically terminated, a latency of 25 sec was scored on that trial.

Results and Discussion

The results are shown in Figure 1. An analysis of variance was carried out on the latency data, using a Treatment × Trials design. The analysis yielded a significant overall groups effect, $F(1, 12) = 23.40$, $p < .01$, indicating shorter latencies for the group that had received inescapable shock. In addition, a significant Group × Trials interaction, $F(29, 238) = 1.70$, $p < .01$, was found, indicating that the animals given inescapable shock showed more rapid acquisition than did no-shock animals. Also a significant trials effect, $F(29, 348) = 5.03$, $p < .01$, was found, indicating that animals learned the nosing response. The superior performance of the animals given inescapable shock was confirmed by examination of avoidance responses (responses in less than 5 sec) as these animals made more avoidances ($M = 19.2$) than did no-shock controls ($M = 7.1$, $t = 5.3$, $p < .001$).

The basis for the superior performance of animals that had received inescapable shock was apparent on observation. The no-shock animals were quite active between trials, frequently moving back and forth, struggling, and changing positions within the restraining tube. The nosing response made by an animal in this group was a very discrete, active response. With the removal of the door over the hole, such an animal would thrust its head forward to complete a correct response, then jerk it back into the restraining tube and resume its intertrial activity. In contrast, the animals that had received inescapable shock remained relatively immobile between trials, and, most significant, their nosing responses were much more passive in nature. As trials progressed, such an animal would come to remain with its nose pressed against the door that covered the hole so that when this door was drawn back at the beginning of a trial, its head followed the door forward, accomplishing a rapid correct response. Following this response, the door moved forward to cover the hole, simply pushing the animal's nose back into the restraining tube into position for a similar correct response on the next trial. The animal normally remained in this position so that it then made a similar rapid correct response on the next trial. In short, animals that had received

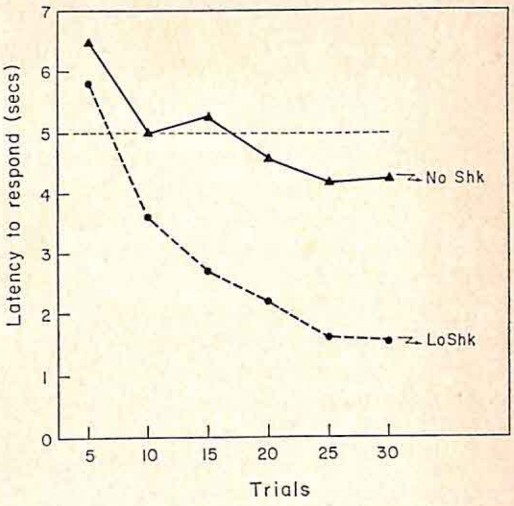

FIGURE 1. Mean latency to perform the correct response (moving nose forward) in the nosing task following the onset of the warning signal (conditioned stimulus) for all groups. (Latency is plotted in blocks of five trials, with the last trial in each block indicated on the abscissa. The LoShk group received long-duration (6-sec), moderate-intensity (1-mA) inescapable shock 72 hr. before this test. The NoShk group was given no shock before this test.)

inescapable shock performed better than no-shock animals because the nosing task was most effectively performed by an animal that remained quietly positioned at the front of the tube, a response facilitated by inactivity.

The results of this experiment showed that animals given inescapable shock can learn an avoidance-escape response better than no-shock controls. This result seems most difficult for the learned helplessness hypothesis to explain; if animals became helpless (learned noncontingency) because of exposure to inescapable shock, they should not have subsequently learned and performed the nosing response better than no-shock controls. On the other hand, if they had learned to be inactive because of exposure to inescapable shock, they would have been able to perform better in a task in which learning was facilitated by inactivity. The results, therefore, argue against the view that animals learned to be helpless as the result of long-duration, moderate-intensity, inescapable shock. The results are compatible with the hypothesis that animals learned to be inactive by exposure to the inescapable shock.

EXPERIMENT 2

This experiment studied the generality of the result found in Experiment 1. Even though the previous experiment indeed showed that an inescapable shock procedure could produce better learning of an avoidance-escape task, it might be possible that the inescapable shock procedure used in that experiment was somehow idiosyncratic, unlike other procedures that have shown the long-term interference effect. If this were true, the conclusion reached at the end of Experiment 1 would not be valid since the inescapable shock procedure used in that experiment would then not relate to the interference effects seen in the previous experiments by other investigators.

To evaluate the question raised above, we repeated the procedure of Experiment 1, at the same time testing for the interference effect by all known procedures that have been used to show the long-term interference effect. Maier et al. (1973), apparently the first to report this type of interference effect in rats, found that inescapable shock interfered with responding in a shuttle using conventional scrambled shock in a task in which the animals had to make two crossings (i.e., from one side of the box to the other and back again) to complete a correct response. Seligman and Beagley (1975) subsequently reported a deficit in responding after inescapable shock when they tested animals by requiring them to press a lever three times to terminate a shock that was administered through a grid floor as one shock contact and a pin in the back as the second shock contract. We have reported (Glazer & Weiss, 1976) that inescapable shock interferes with simple shuttle responding in our shuttle apparatus, using a neonbulb shock system. These, then, are the three test conditions used to ascertain interference effects thus far: FR-2 (fixed ratio) shuttle (scrambled shock) by Maier and co-workers, FR-3 bar-press (pin and floor contact) by Seligman and co-workers, and normal (one-response) shuttle (neon-bulb shock) by Glazer and Weiss.

To determine whether the inescapable shock procedure that led to better learning in the nosing task (Experiment 1) was in fact representative of the procedures that others as well as ourselves have used to produce interference effects, we exposed animals to the three test conditions that have yielded interference effects and used the nosing apparatus. That is, after animals were exposed to inescapable shock (LoShk) one group was tested in an FR-2 shuttle with scrambled grid shock as described by Maier et al. (1973), one group was tested in an FR-3 lever press with "grid floor, pin-in-back" shock as described by Seligman and co-workers, one group was tested in our shuttle with neon-bulb shock, and one group was tested in the nosing task. Each of these groups was compared with a no-shock control group in the same task. The aim of this study was to determine whether the LoShk procedure would (a) produce deficits in all the tests used to ascertain the presence of the long-term interference effect and (b) enhance learning in the nosing task. If this proved to be true, the generality of the conclusion at the end of Experiment 2 would be established.

Method

Subjects. The subjects were 56 naive rats as described in Experiment 1.

Apparatus. The apparatus for the delivery of inescapable tail shocks was as described in Experiment 1. The descriptions of the avoidance-escape testing apparatuses are as follows: The barrier jump test apparatus was a two-compartment shuttle box described in Glazer and Weiss (1976). The nosing apparatus was the same as that described in Experiment 1. The FR-3 bar-press apparatus was closely patterned after that described in Seligman and Beagley (1975). A Plexiglas chamber, 12 in. long × 7 in. wide × 12 in. high, (30.4 × 17.8 × 30.4 cm), housed in a sound-attenuated cubicle was used. The chamber was lit by a 1.5-W light. Centered on the left wall was a lever measuring 2 in. × 1.5 in. (5.1 × 3.8 cm) mounted 2 in. (5.1 cm) off the floor, which was the manipulandum in the FR-3 escape test. The floor was made of stainless-steel grid bars. Shock was delivered to the animal through this grid floor (one pole) and a safety-pin electrode (other pole) placed subcutaneously through the back of the rat. A relatively constant-current shock of 1 mA intensity was delivered from a 600-V transformer through a resistor. Shock pulsed on and off 5 times/sec. The FR-2 shuttle apparatus was patterned after that described in Maier et al. (1973). A 12 in. × 12 in. × 12 in. (30.4 × 30.4 × 30.4 cm) Plexiglas chamber, housed in a sound-attenuated cubicle, was used. The chamber was lit with a 1.5 W light and divided in half lengthwise by a metal divider extending over its entire height and width. A rounded open archway, 3 in. (7.6 cm) high and 2 in. (5.1 cm) wide, was cut out of the middle of the divider. Thus, to cross from one side of the chamber to the other, the animal had to pass through a hole. A speaker mounted on the roof was used to deliver a 1,000-cps, 80-dB tone stimulus (CS). A 1-mA shock could be delivered to the grid floor through a Grason Stadler scrambled-shock generator.

Procedure. The subjects were divided into eight groups of seven animals each. Four groups of subjects were given inescapable electric shocks delivered to the tail. A total of 60 shocks, 1 mA in intensity and 6 sec in duration, were delivered on a 1-min. schedule (variable time). The remaining four groups received no inescapable shock. Two groups, one that received inescapable shock and one no-shock group, were then tested in each one of the four test procedures. For the groups that received inescapable shock, the test was given 72 hr. after the shocks.

For animals tested in the nosing task, the procedure was as described in Experiment 1. For those animals tested in the barrier jump, the procedure was as described in Glazer and Weiss (1976). Presentation of a 1,000-cps, 80-dB tone (CS) initiated a trial. After 5 sec of tone, shock (.3 mA measured with no rat on the grid) was delivered through the grid floor and barrier. If the animal jumped the barrier during the tone prior to the shock, the tone was terminated (avoidance response) and a 1-min. ITI was initiated. If the animal crossed the barrier following shock onset, then both tone and shock were terminated (escape response) and the ITI was initiated. If the animal failed to cross the barrier after 20 sec of shock, the shock was automatically terminated and the ITI was initiated. The test consisted of 30 trials, and the data collected were latencies to shuttle as measured from the onset of the tone. If the animal did not respond within 25 sec, so that the trial was automatically terminated, a latency of 25 sec was scored on that trial.

For animals tested in the FR-2 shuttle task, a procedure similar to that described in Maier et al. (1973) was used. The tone initiated the trial, followed 5 sec later by shock delivered to the grid floor. Correct responses occurring before the shock terminated the tone and initiated the 1-min. ITI (avoidance response), while responses after shock onset terminated shock and tone and initiated the ITI (escape response). As in the Maier et al. (1973) procedure, on the first 5 trials, a single crossing terminated the shock. Following these 5 trials, the FR-2 response requirement (two crossings—first to one side and then back) was instituted for the next 25 trials. If an animal did not correctly respond (perform the two crossings) within 30 sec of shock onset, the shock and tone were automatically terminated. Latency to respond correctly, from CS onset, was measured. If the animal did not respond within 35 sec, so that the trial was automatically terminated, a latency of 35 sec was scored on that trial.

In the FR-3 lever-press test, a procedure similar to that described in Seligman and Beagley (1975) was used. For this procedure, a trial was initiated by presentation of shock. Three lever presses terminated the shock and initiated the ITI, which was 1 min. (variable time). No warning signal preceded the shock. If an animal had not completed three lever responses 60 sec after shock initiation, the shock was automatically terminated and the ITI initiated. The test consisted of 30 trials. Latency to perform the FR-3 response from shock onset was measured. If the animal did not respond within 60 sec, so that the trial was automatically terminated, a latency of 60 sec was scored on that trial.

Results and Discussion

Figure 2 shows the results for each of the four test responses studied in this experiment, with each section showing mean latency to respond plotted over blocks of

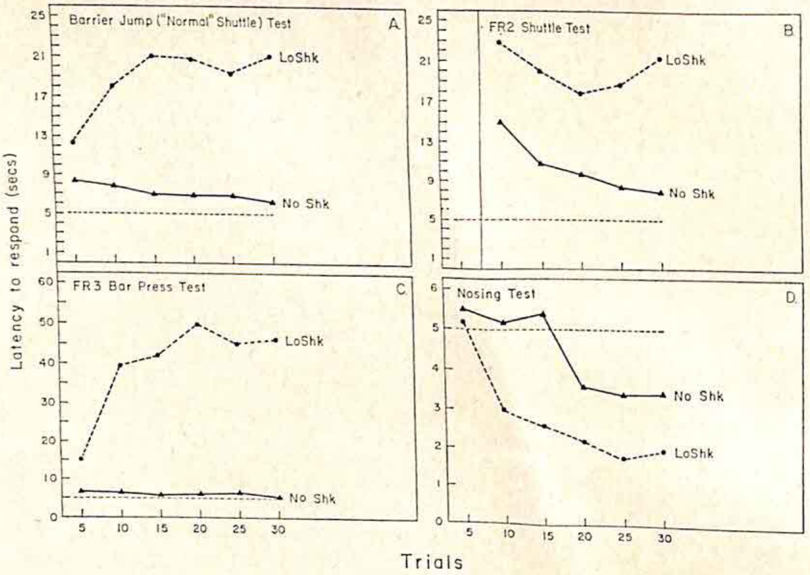

FIGURE 2. Mean latency to perform the correct response following onset of the warning signal (conditioned stimulus) in four different tasks. (Latency is plotted in blocks of five trials, with the last trial in each block indicated on the abscissa. The LoShk groups were exposed to long-duration (6-sec), moderate-intensity (1-mA) inescapable shock 72 hr. before test. The NoShk groups were given no shock before the test. Section A shows results in a standard ("normal") shuttle task as used by Glazer and Weiss (1976), Section B shows results in a two-response shuttle test as used by Maier et al. (1973), Section C shows results in a three-response lever-press test as used by Seligman and Beagley (1975), and Section D shows results of a nosing test.)

five trials. The results of each test response were analyzed separately by a Treatment (inescapable shock vs. no shock) × Trials analysis of variance. These analyses of variance yielded significant treatment, trials, and Treatment × Trials effects for all four test responses as follows: For nosing, $F(1, 12) = 14.23$, $p < .01$; $F(29, 348) = 4.47$, $p < .01$; and $F(29, 348) = 1.94$, $p < .01$ respectively. For barrier jump, $F(1, 12) = 38.61$, $p < .01$; $F(29, 348) = 2.59$, $p < .01$; and $F(29, 348) = 2.83$, $p < .01$, respectively. For FR-2 shuttle, $F(1, 12) = 17.24$, $p < .01$; $F(29, 348) = 12.62$, $p < .01$; and $F(29, 348) = 12.00$, $p < .01$, respectively. For FR-3 lever press, $F(1, 12) = 36.86$, $p < .01$; $F(29, 348) = 8.93$, $p < .01$; and $F(29, 348) = 3.87$, $p < .01$, respectively.

For all test responses except the nosing response, animals that received shock showed longer latencies than did no-shock control animals. In short, the interference effect showed up clearly in all three of the test responses in which it was expected. The data from the shuttle test replicated our previous results (Glazer & Weiss, 1976), while the data from the FR-3 lever-press and FR-2 shuttle closely followed the data presented respectively by Seligman and Beagly (1975) and Maier et al. (1973). It can therefore be stated with some confidence that the inescapable shock procedure used in our experiments is not idiosyncratic and that the phenomenon we have been studying is the same phenomenon that Seligman, Maier, and their co-workers have been studying.

The most important finding, then, is that the same inescapable shock procedure that produces the interference effect seen in our studies, those of Seligman and co-workers, and those of Maier and co-workers, produces better learning in the nosing avoidance-escape task. This is seen in Part D of Figure 2. This conclusion is also confirmed if one looks at avoidance (less than a 5-sec latency) responses, as animals given inescapable shock made more ($M = 23.4$) avoidances than did no-shock controls (M

$= 11.0$; $t = 4.2$, $p < .001$). As stated at the conclusion of Experiment 1, such a result indicates that animals given inescapable shock did not learn to be helpless as a result of the shock. The results remain consistent with the view that animals had learned to be inactive.

EXPERIMENT 3

Throughout the discussions of the learned helplessness hypothesis, much emphasis has been placed on the fact that the interference effect results from lack of control over shock and not from shock per se. When pairs of animals were studied (Seligman & Beagley, 1975; Seligman & Maier, 1967) in an avoidance-yoked animal paradigm, the animals that received inescapable shock (yoked animals) subsequently showed the interference effect, whereas animals that received the same shocks but controlled them (avoidance-escape animals) did not show the interference effect. This difference between avoidance-escape and yoked animals has been shown with respect to very strong shocks (transient interference effect) by Seligman and Maier (1967), and also with respect to long-duration, moderate-intensity shocks (long-term interference effect) by Seligman and Beagley (1975). The proponents of learned helplessness have claimed that the avoidance-yoked difference shows that "lack of control over shock" resulting in learned helplessness is indeed the basis for the interference effect.

However, alternatives to learned helplessness are also consistent with the fact that avoidance-escape animals show no interference effect, whereas yoked animals do. Regarding the transient interference effect, Weiss, Glazer, and Pohorecky, (1976), and Weiss, Stone, and Harrell (1970) showed that yoked animals given strong inescapable shocks manifested depletion of brain catecholamines that avoidance-escape animals receiving the same shocks did not, and that such a difference could explain the transient or "motor activation deficit."

With respect to the long-term interferference effect, the hypothesis advanced in the present article—that animals learned to lower their activity level as a result of exposure to the moderate, long-duration shock—is also consistent with a deficit in yoked animals that is not found in avoidance-escape animals. This explanation is stated as follows: The long-term interference effect is said to depend on an animal being inactive at the instant of shock termination. Avoidance-escape animals therefore should not show an interference effect because in an active avoidance task by definition they must be active at the instant of shock offset in performing the avoidance-escape response. In contrast, a yoked animal could very well be inactive at shock termination if the avoidance-escape animal took sufficiently long to terminate the shock. Based on our earlier data (Glazer & Weiss, 1976) showing that the interference effect appeared with a moderate-intensity shock of 5 sec duration or longer, a yoked animal would be expected to show the interference effect if its matched avoidance-escape animal generally took 5 sec or longer to terminate shocks. Thus, the "learned inactivity" hypothesis can explain why yoked animals and not avoidance-escape animals will show the long-term interference effect but states that yoked animals must receive shocks of 5 sec or more in duration for this to occur.

It is important to note, then, that in the one experiment that showed the long-term interference effect in yoked rats as contrasted with avoidance-escape rats (Seligman & Beagley, 1975, Experiment 4), this study used an initial shock condition in which avoidance-escape animals received many long shocks before performing the correct response. The average duration of shock proved to be 5.5 ± .3 sec in duration, so that learned inactivity could account for these results of Seligman and Beagley.

The purpose of the present experiment was twofold. First, we sought to determine whether we could demonstrate an interference effect for animals that received inescapable shocks(i.e., yoked animals) that would not be present for animals that could avoid/escape the same shocks (avoidance-escape animals). For this study, we tested for an interference effect by using the FR-3

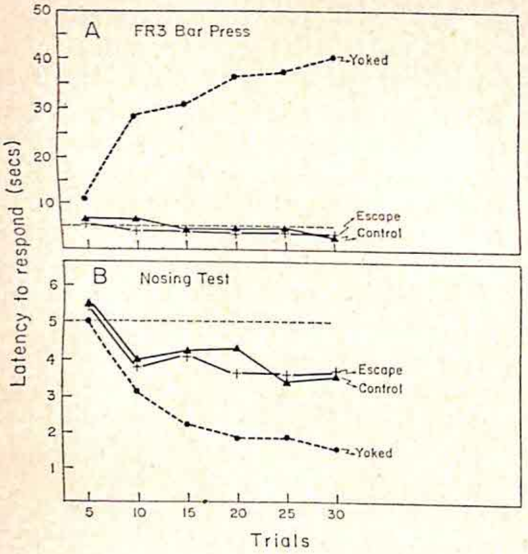

FIGURE 3. Mean latency to perform the correct response following the onset of the warning signal (conditioned stimulus) in two different tasks. (Latency is plotted in blocks of five trials, with the last trial in each block indicated on the abscissa. The escape animals were allowed to escape from shock during a shock session 72 hr. before the test. The yoked animals were matched to escape subjects; they received the same shocks as escape subjects but were unable to escape from them, i.e., they received inescapable shock 72 hr. before the test. The control animals received no shock before the test. Section A shows results in an FR-3 (fixed ratio) lever-press task as used by Seligman and Beagley (1975), and Section B shows results in a nosing task.)

lever-press response so there would be no question that the phenomenon being studied was the same as shown by Seligman and coworkers. Second, and perhaps most important, we also tested other matched avoidance-escape and yoked animals in the nosing task. The purpose of this part of the study was to determine whether yoked animals would actually show *better* learning than avoidance-escape animals in the nosing task, as would occur if yoked animals had learned a reduction in activity rather than "helplessness."

Method

Subjects. The subjects were 42 naive rats as described in Experiment 1.

Apparatus. The apparatus for training triplets (avoidance-escape, yoked, control) have been described in detail elsewhere (Weiss, 1971). This apparatus consists of three small boxes each in its own soundproof chamber. In each box an animal could be confined with its tail protruding out of the box so that shock electrodes could be attached to the tail. A wheel was mounted on the front wall of each box that the animal could rotate; rotations of this wheel could be made to terminate or avoid the tail shock or could be made inoperative with respect to the shock. The test apparatus consisted of the nosing avoidance-escape apparatus described in Experiment 1, and the apparatus used for the FR-3 level-press test was that used in Experiment 2.

Procedure. The animals were run in triplets (three rats simultaneously) in the apparatus described in Weiss (1971). The detailed description of how tail guards and electrodes were affixed to the animals may be found in Weiss (1971). Two animals were wired with tail shock electrodes in series, while the third animal was prepared identically with the tail electrodes but did not receive any shock. The avoidance-escape animal of the triplet was trained as follows: At the start of the session, trials were presented at the rate of 1/min (1-min. fixed-time schedule). A trial began with the presentation of shock. The animal could escape (terminate) the shock by making one complete revolution of a 4-in. (10.2 cm) diameter wheel. The length of the shock before an escape was possible was progressively increased, starting at .1 sec and progressing from .1, 1.0, 2.0, 3.0, 4.0, 5.0, 6.0, to 7.0 sec. The criterion for increasing shock duration was completion, on three successive trials, of the escape response within 1 sec from the time that a response would be effective. Increases in shock duration were also accompanied by increases in shock intensity. Shock intensity was set at .5 mA with the .1- and 1.0-sec delay, .6 mA with 2.0- and 3.0-sec delay, .8 mA with the 4.0- and 5.0-sec delay, and 1.0 mA with the 6.0- and 7.0-sec delay. The shaping time for avoidance-escape animals to reach the 7-sec delay and 1.0-mA intensity was approximately 25–35 min. Once the avoidance-escape animals were performing so as to terminate the 1.0-mA shock almost immediately after 7 sec had elapsed from shock onset, the schedule of shock delivery was changed from a trial regularly each minute to a 1-min. variable-time schedule. Animals then remained on this schedule for 1 hr.

Yoked animals received the same shocks as did the avoidance-escape animals during all phases of the experiment described above. The wheel was available to these subjects, but it was locked in position so that it could not be turned. No-shock controls received no shock throughout the procedure. A total of 14 triplets were trained in this manner. Following this training, all animals were returned to their home cages. Seventy-two hours later, all subjects received avoidance-escape testing. Half of the triplets (seven) were tested in the FR-3 lever-press task as described in Experiment 2 and the remaining half in the nosing task as described in the same experiment.

Results and Discussion

Figure 3 presents average latencies to respond for the three groups over blocks of five trials for nosing and the FR-3 lever-press responses, respectively. Analyses of variance were done separately for the two test responses using treatment (avoidance-escape, yoked, control) and trials as factors. Both of the analyses yielded significant treatment, trials, and Treatment × Trials effects. For nosing, $F(2, 18) = 9.40$, $p < .01$; $F(29, 522) = 6.86$, $p < .01$; and $F(58, 522) = 2.33$, $p < .01$, respectively. For FR-3 lever-press, $F(2, 18) = 26.87$, $p < .01$; $F(29, 522) = 7.24$, $p < .01$; and $F(58, 522) = 8.26$, $p < .01$, respectively. Summarizing these results, in the FR-3 lever-press test, both the avoidance-escape and no-shock groups showed short latencies and a pattern of declining latencies over trials, whereas the yoked animals showed long latencies with a pattern of increasing latencies over trials. In the nosing test, all groups showed declining latencies; however, the yoked animals showed the shortest latencies overall and more rapidly declining latencies over trials than the other groups.

The data showed that the responding of yoked animals was clearly deficient in the FR-3 lever-press test in comparison with avoidance-escape and control animals. These results therefore replicate the "classic" effect of the learned helplessness hypothesis. However, the results also showed that when the test response required only minimal movement on the part of the subject (nosing), the performance of such yoked animals was reliably ($p < .001$) superior (M avoidance responses $= 25.3$) to that of either avoidance-escape (M avoidances $= 15.7$) or control animals (M avoidances $= 13.0$). The results seem incompatible with the view that the animals which received inescapable shock (yoked animals) learned to be helpless. On the other hand, the findings are compatible with the idea that the long-term interference effect results from learning of lower activity levels.

General Discussion

This article presents three experiments dealing with the nature of the interference effect. The original explanation offered for this effect was called the learned helplessness hypothesis. This hypothesis proposes that animals subjected to inescapable electric shocks learn that there is no contingency between their behavior and termination of shock and therefore give up any subsequent attempts to control shock. Proponents of the helplessness theory maintain that both the earlier demonstrations of interference using dogs as subjects and the more recent demonstrations with rats are representative of learned helplessness.

It is our contention that two distinct types of interference effects exist. The first type of interference arises from the organism's inability to activate motor systems required to perform the avoidance-escape response in the test situation. This motor activation deficit is transient in nature and is mediated by disturbances in central neurotransmitters induced by traumatic electric shock (Glazer et al., 1975; Weiss & Glazer, 1975; Weiss et al., 1975, 1976). The data from the dog experiments (e.g., Overmier & Seligman, 1967; Seligman & Maier, 1967) and our earlier experiments with rats (e.g., Weiss & Glazer, 1975) are representative of this phenomenon. More recent data (Glazer & Weiss, 1976; Maier et al., 1973; Seligman et al., 1975) point to the existence of a second type of interference effect that is nontransient in nature and is based on learning. The present experiment deals with this latter phenomenon.

As noted by Glazer and Weiss (1976), the motor response of animals to long-duration, moderate-intensity inescapable tail shock appears to be biphasic in nature, showing a peak of activity at the time of shock initiation followed by a decline in the amount of movement as shock continues. These observations, coupled with the data showing that the interference effect will appear only if the inescapable shocks are 5 sec or longer in duration (Glazer & Weiss, 1976), led us to suspect that in the recent experiments with rats, animals receiving inescapable shocks did not learn noncontingency (i.e., helplessness) but rather acquired the tendency to be inactive.

The results reported in the present article are consistent with the view that animals given long-duration, moderately intense inescapable shock learned to be inactive. The results are not consistent with expectations derived from the helplessness hypothesis. The present experiments demonstrated that (a) animals receiving inescapable shock could learn and perform an avoidance-escape response that required little movement—a nosing response—better than no-shock controls; (b) the same inescapable shock condition that interfered with the performance in all tasks used to delineate the interference effect (barrier jumping, FR-2 shuttle, and FR-3 lever-press) potentiated performance of this nosing avoidance-escape response; and (c) in an avoidance-yoked paradigm, yoked animals showed a deficit in the subsequent performance of the FR-3 lever-press escape response but showed superior performance in the nosing test.

Maier and Testa (1975) and Seligman et al. (1975) have described what can be called supplementary findings that are claimed to support the learned helplessness hypothesis. It is our contention that such supplementary findings are explained by "learned inactivity" as well and thus present no particular support for the learned helplessness hypothesis.

For example, Seligman et al. (1975) showed that, if they pretrained rats to jump up on a platform to escape shock, these animals would not show a deficit when they were subsequently given inescapable shock and then tested in the FR-3 lever-press escape task. This result was said to show that pretraining "immunizes" animals against learning to be helpless. However, the effect of pretraining can also be explained if inescapable shock causes animals to learn to lower their activity level. According to this view, pretrained animals should not show a deficit because such animals are thereby trained to be active (to jump in response to shock), so they have specifically learned not to become inactive in response to shocks. It is significant to note that Seligman et al. conducted all three phases of their study (jump-up escape, inescapable shock, and FR-3 escape) using the same apparatus; thus, one would indeed expect the original learned activity responses to generalize considerably to the subsequent FR-3 lever-press escape task and thus prevent a deficit from occurring.

In another experiment, Seligman et al. (1975) demonstrated that animals given inescapable shock will learn an FR-3 lever-press escape response when repeatedly picked up and dropped onto the lever so they experience that a correct response terminates shock. Since this simply demonstrates the effectiveness of a retraining procedure, the findings do not especially support learned helplessness. The results are equally compatible with the view that animals were being taught to reinitiate active behavior after having learned to be inactive. The results described in Maier and Testa (1975) can be similarly explained by such alternative means.

The final point that requires comment in this article concerns the original dog experiments carried out by Seligman, Maier, and their co-workers. The shock procedure used in these dog studies clearly produced a transient avoidance-escape deficit, or "motor activation deficit" in our terminology. The transient nature of the original effect, characterized by a disappearance of the deficit within 2–3 days, is firm; this was seen in two separate studies (Overmier, 1968; Overmier & Seligman, 1967) and can be contrasted with the fact that both Seligman et al. (1975) and Glazer and Weiss (1976) have now shown that the interference effect in rats receiving LoShk lasts at least 1 week with no indication of any decay with the passage of time. However, given that the original studies showed a transient effect, this raises the question of why a long-term effect was not found in those studies since the duration of inescapable shock was 5 sec. The answer to this question can only be speculative, but suggestions can be made. First, 5 sec was the shortest duration of shock that Glazer and Weiss found to be effective in producing any long-term effect, so the dog experiments are a "borderline" case. Moreover, the present experiments, showing that shock of 5 sec duration produces a long-term effect, have used rats as subjects, and rats appear to have a greater propensity to freeze in fear situations than do dogs. Since the long-term effect may be based on learned inactivity, this species difference could well be an important factor in resolving this issue. Nevertheless, a definitive answer to the question remains to be given; at present, we can simply note that with the shock conditions and subjects used in the studies by Overmier and Seligman (1967) and Seligman and Maier (1967) (6-mA intensity shocks, 5 sec duration, dog subjects), the interference effect was transient even though one of the parameters was shock of 5 sec duration.

In summary, we believe that animals given long-duration, moderate-intensity electric shocks learn, as a result of this experience, behavior that interferes with the subsequent performance of avoidance-escape responses requiring appreciable amounts of motor activity. We have referred to the competing behavior learned as learned inactivity, but whether the animal actually learns to be inactive or learns some behavior that makes it appear inactive (such as, e.g., gripping the grid floor) is not clear at this time. The important point is that our results suggest the interference effect arises because the animals acquire and then actively practice a competing behavior. We do not subscribe to, nor feel the data presented as yet support, the idea that the interference effect occurs because rats or dogs learn to "give up" and/or are deficient in forming associations.

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Stimulus Quality and Assessments of Relative Importance of S+ and S−

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Three experiments were performed to test the assertion that attractiveness of novel or complex stimuli has confounded experiments on the relative importance of S+ and S− in discrete-trial discrimination. In the first, rats which had learned a visual discrimination were given replaced-stimulus transfer tests with either S+ or S− replaced by either a homogeneous gray or a black and white checkerboard stimulus. A significant interaction was found; with the checkerboard, replacement of S− produced more errors than replacement of S+, but with the gray stimulus, replacement of S+ produced more errors than replacement of S−. In the second experiment, novelty of a replacement stimulus was manipulated through preexposure. Again, there was an interaction; with the novel stimulus, replacement of S− produced more errors than replacement of S+, but with the familiar stimulus, the opposite was found. In the third experiment, rats learned a discrimination more rapidly when S+ was more distinctive than S− as compared to when S− was more distinctive than S+. Together these findings indicate that the results of tests used to assess the relative degree of control by S+ and S− may be due to nonassociative factors.

Various attempts have been made to assess the relative importance of S+ versus S− in simultaneous discrimination learning. These attempts can be divided into two basic types: those which use replaced-stimulus transfer tests and those which manipulate stimuli during acquisition. Both of these methods are subject to bias resulting from confounding by nonassociative variables.

For example, the most frequent method of assessing the relative importance of S+ and S− in simultaneous discrimination with rats is the replaced-stimulus transfer test. Here the animal acquires a discrimination and then is tested with either S+ or S− replaced with a new stimulus. The differential performance between the two test conditions is taken to measure the relative importance of the correct versus the incorrect stimulus. Using this method, a significantly greater frequency of errors was found with S− replaced than with S+ replaced following various stages of training: premastery, criterion, over-training, and reversal (Hall, 1973; Mandler, 1968; Stevens & Fechter, 1968; Stevens & Wixon, 1975). This method is susceptible to a serious confounding: If animals were approaching the replaced stimulus because of its novelty, results similar to those obtained would be predicted, since in the S+ replaced condition an animal approaching the novel stimulus would be making a "correct" response, and in the S− replaced condition an animal approaching the novel stimulus would be making an "incorrect" response. Thus, if animals respond in part on the basis of novelty in a replaced-stimulus transfer test, the results could erroneously suggest that S− was the more important stimulus.

Another method of assessing the relative importance of S+ and S− is the multiple-stimulus training task. With this method animals are trained with either multiple S+s and a constant S−, or a constant S+ and multiple S−s. If the group having a constant S− learns more efficiently than the group with a constant S+ this result is interpreted as evidence for S− being relatively more

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important than S+ (Mandler, 1970, 1971). Opposite results are given the opposite interpretation (Gardner & Coate, 1965). This method is also susceptible to nonassociative confounding. If animals are responding to (i.e., approaching) the changing or novel stimuli, this would result in fewer errors when S+ was the changing stimulus (constant S−) than when S− was the changing stimulus (constant S+). Thus, more efficient "learning" with multiple S+s may not reflect the importance of S− as much as the incentive value of multiple S+s.

If the argument presented here regarding the confounding factor in replaced-stimulus tests is correct, then the manipulation of stimulus quality will produce differential performance in a transfer task. Accordingly, Experiment 1 investigated replaced-stimulus transfer test performance using stimuli of different levels of complexity as the replacement stimuli.

EXPERIMENT 1

Method

Subjects. The subjects were 80 male rats of the Charles River CD strain, about 70 days old.

Apparatus. The apparatus was a discrimination box similar to that described by McGaugh and Thomson (1962, fig. 1). It was trapezoidal in shape, 27 in. (68.5 cm) long, 12 in. (30.5 cm) wide at one end and 6 in. (15.2 cm) wide at the other end, with a grid floor. A Plexiglas door 7 in. (17.8 cm) from the narrow end provided a startbox. Two openings, 3 in. (7.6 cm) square and 6 in. (15.2 cm) apart were located in the wall at the wide end. A 3-in. (7.6-cm) partition separated the openings. Swinging doors, which served as stimuli, were located behind these openings. One door opened into a goalbox and the other was locked shut. In the goalbox was a small cylinder which contained 0.2-ml water. The entire apparatus was painted medium gray. To provide an objective determination of errors (responses to the incorrect door), a microammeter was used. One pole was wired via a double-throw switch to the 3-in. (7.6-cm) grid section in front of the incorrect door, and the other pole was wired in series with a dry cell and the remainder of the grid. When a subject touched the floor in front of the incorrect door, it was indicated by deflection of the needle of the meter. The current passed ($< 10\ \mu A$) was not aversive and was probably subliminal.

Four different stimuli were used. Two were doors painted with alternating black and white ½-in. (1.3-cm) stripes, one oriented horizontally, one vertically. The third was a pattern of alternating black and white quadrants, each 1.5 in. (3.8 cm) square, that is, a 4-cell checkerboard. The fourth stimulus was a homogeneous medium-gray door.

Procedure. The general design of the experiment involved the acquisition of a vertical versus horizontal stripe discrimination followed by a replaced-stimulus transfer test in which either S+ or S− was replaced by either the gray or checkerboard stimulus door. The experiment was run in two replications, each using 40 subjects. For the first, the vertically striped door was S+ and the horizontally striped door was S−. For the second replication, the horizontally striped door was S+ and the vertically striped one was S−. Within each replication, 10 subjects were randomly assigned to each of the four replaced-stimulus test groups.

The subjects were gentled and adapted to a 23-hr 50-min water deprivation schedule, and then pretrained. Those subjects assigned to be tested with the gray stimulus were pretrained with the checkerboard door, and the subjects assigned to be tested with the checkerboard stimulus were pretrained with the gray door. Initially the correct door was propped open, later it was fully closed. A block was placed in front of the incorrect door forcing the subject to respond correctly on every trial. Ten trials a day were given until the subjects left the start box upon the opening of the guillotine door, opened the stimulus door, and entered the goal box without delay. This took from 40 to 50 trials. Gellerman sequences were used in this and subsequent phases of the experiment to determine the position of the correct stimulus.

Following pretraining, the subjects were trained on the vertical versus horizontal stripe discrimination. Ten trials a day were given until a criterion of nine correct choices within a day was met. On the day following the attainment of the criterion the rats were tested with either S+ or S− replaced by either a gray or a checkerboard stimulus. Two subjects, both to have been tested with gray replacing a horizontal S−, were removed from the experiment during training due to excessive balking.

Results and Discussion

Acquisition. Mean errors to criterion made during acquisition are shown in Table 1, grouped according to the orientation of S+ (replication) and subjects' assignment to subsequently given replaced-stimulus tests. Rats trained with horizontal stripes as S+ learned with fewer errors than those trained with vertical stripes as S+. A two-way ANOVA was performed on the error scores. A significant effect was found for orientation of S+, $F(1, 70) = 12.03$, $p < .001$, but not for the comparison of subsequent test groups, $F(1, 70) = 0.68$, nor for the interaction of

TABLE 1

Mean Acquisition Errors for Experiment 1 Grouped According to Orientation of Training Stimuli and Subsequent Test Conditions

Replacement stimulus	Orientation of S+	
	Vertical	Horizontal
Gray		
S+	30.3	28.5
S−	39.9	25.6
Checkerboard		
S+	34.7	29.8
S−	35.8	21.0

orientation of S+ and test groups, $F(1, 70) = 1.63$.

Replaced-stimulus tests. The test results, presented in Figure 1, indicated an interaction of type of replacement stimulus and whether it replaced S+ or S−. When a checkerboard stimulus was used, replacement of S− produced more errors than replacement of S+, while the opposite relationship was found when a gray stimulus was used. A three-way ANOVA indicated that this interaction was significant, $F(1, 70) = 11.61$, $p < .002$. With replacement of gray stimuli, more errors were made by groups trained with the vertically striped stimulus correct than by groups trained with the horizontally striped stimulus correct, but no such difference was found with replacement by checkerboard stimuli. This interaction yielded $F(1, 70) = 4.77$, $p < .04$. No other significant effects were found. For the main effect for orientation of training stimuli, $F(1, 70) = 1.81$, for replacement of S+ versus S−, $F(1, 70) = 2.64$, and for gray versus checkerboard replacement stimuli, $F(1, 70) = 0.38$. For the interaction of orientation of training stimuli and replacement of S+ versus S−, $F(1, 70) = 0.62$, and for the three-way interaction, $F(1, 70) = 0.05$.

Since there was a significant two-way interaction between type of replacement stimulus and replacement of S+ versus S−, together with no evidence of a three-way interaction, the data from groups trained under both orientations of S+ were pooled and differences between groups tested with S+ or S− replaced were compared separately for the gray and checkerboard replaced-stimulus conditions using Tukey's LSD method. This analysis showed that with checkerboard stimuli, significantly fewer errors were made when S− was replaced than when S+ was replaced ($p < .01$), but with gray stimuli the difference between the two replaced-stimulus groups was not statistically significant. This finding is consistent with the assertion that nonassociative effects varying with stimulus quality can confound associative effects in replaced-stimulus transfer tests.

This interaction would be expected if there was differential generalization from training stimuli to replacement stimuli varying as a function of both the orientation of training

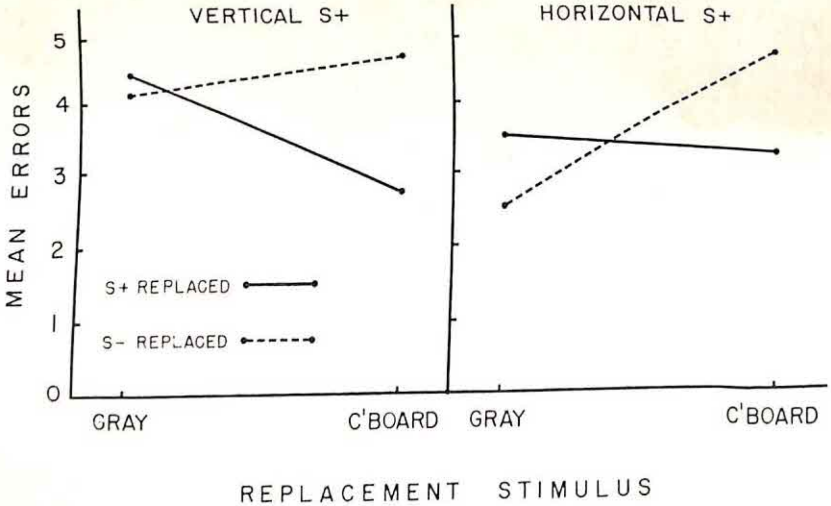

Figure 1. Mean frequency of errors made with S+ or S− replaced, plotted separately for type of replacement stimulus and orientation of training stimuli. (C'board = checkerboard.)

stimuli and their role as S+ or S−. However, the lack of a three-way interaction indicates no such differential generalization.

Still another associative explanation for the interaction between stimulus quality and transfer test performance is possible. In the course of acquisition the subjects may have learned not to approach the gray walls of the apparatus. This learned response to gray surfaces could account for the greater frequency of errors made when a gray stimulus replaced S+ than when a gray stimulus replaced S−. Consequently the interaction obtained might reflect differential generalization from the gray walls to the checkerboard and gray stimuli used for transfer tests.

The interaction between orientation of S+ and type of replacement stimulus is irrelevant to any interpretations of the differential performance found when S+ or S− is replaced since it does not reflect replacement of stimuli. The interaction cannot be attributed to differences in acquisition, differential generalization between training stimuli and replacement stimuli, or incentive properties of the replacement stimuli. Possibly some unknown factor operated differentially between replications despite the experimenters' efforts to maintain equal experimental conditions between them.

EXPERIMENT 2

Experiment 1 demonstrated that manipulation of stimulus complexity produces differential results in replaced-stimulus transfer tests. In the introduction the assertion was made that stimulus novelty also is a confounding factor in replaced-stimulus transfer tests. Accordingly, in Experiment 2 the relative novelty of the replacement stimulus was manipulated and its effect on transfer test performance was determined. This was achieved by preexposing one group of subjects to the replacement stimulus and another group of subjects to a stimulus not used in the replacement tests. The extent to which stimulus novelty affects transfer test performance would be represented in an interaction between the cue function of stimulus replaced (S+ vs. S−) and the preexposure to the replacement versus the irrelevant stimulus. The traditional results (more errors with S− replaced than with S+ replaced) should more likely be obtained following preexposure to the irrelevant stimulus than following preexposure to the replacement stimulus. With this design, results are not subject to the interpretation concerning generalization from the gray walls, since gray stimuli were not used in the transfer test.

Method

Subjects. The subjects were 40 male rats of the Charles River CD strain, about 70 days old.

Apparatus. The experimental housing consisted of 10 Alpine cages (#V2240b) each divided in half lengthwise by a solid aluminum partition providing 20 chambers each 20 in. (50.8 cm) long, 11 in. (27.9 cm) wide, and 12.8 in. (32.5 cm) high. Aluminum panels 5 in. (12.7 cm) square were mounted on the rear wall of each chamber, 5 in. (12.7 cm) from the floor. For half of the chambers the panels were homogeneous gray, in the remaining chambers the panels had a black and white four-cell checkerboard 3 in. (7.6 cm) square painted on a gray background. Pairs of cages were placed, one above the other, on tables arranged equally distant from a centrally located fluorescent light fixture. The assignment of homogeneous gray or checkerboard panels to cages was counterbalanced with regard to cage location.

The subjects were tested in the discrimination box described in Experiment 1, with one change: Instead of the correct door opening fully when pushed it was limited to opening .75 in. (1.9 cm), at which point it was held open by a permanent magnet. This opening exposed a water cup containing .2 ml of water.

The vertically and horizontally striped stimuli described in Experiment 1 were used for training.

Procedure. Experimental groups were formed each containing five subjects. These groups composed the eight cells of a 2 × 2 × 2 factorial design. The factors were vertical versus horizontal stripes as S+, replacement of S+ versus S− on test trials, and preexposure to gray versus checkerboard panels. Each subject was placed in the experimental housing where it resided for the course of its participation in the experiment and was gentled for 10 min a day for 5 days. For the first 20 days of the experiment the animals were deprived of water for 23 hr per day. The deprivation schedule was increased to 23 hr 50 min from the 21st day on. Pretraining in the discrimination box began on the 22nd day. Vertically and horizontally striped stimuli were used for pretraining with the two patterns occurring equally often on the right or left. Otherwise, the procedure reported for Experiment 1 was followed. Three subjects were removed from the experiment due to excessive balking during pretraining. These subjects had been assigned to the following groups: vertical S+/checkerboard preexposure/S− replaced; vertical S+/gray preex-

TABLE 2

MEAN ACQUISITION ERRORS FOR EXPERIMENT 2 GROUPED ACCORDING TO ORIENTATION OF TRAINING STIMULI, PREEXPOSURE STIMULUS, AND TRANSFER TEST CONDITION

Experimental treatment	Orientation of S+	
	Vertical	Horizontal
Gray		
S+	62.0	39.3
S−	59.5	45.5
Checkerboard		
S+	58.0	54.8
S−	49.0	44.8

posure/S— replaced; and horizontal S+/checkerboard preexposure/S— replaced.

Acquisition training began on the 29th day. Ten trials a day were given until a criterion of nine correct responses in a daily session was met. On the day following the attainment of the criterion each subject was given 10 test trials with the checkerboard stimulus replacing either S+ or S— according to its experimental group assignment.

Results and Discussion

Acquisition. Mean errors to criterion made during acquisition are shown in Table 2 grouped according to orientation of S+, preexposure stimulus condition, and the subsequently given replaced-stimulus conditions. No significant differences attributable to those conditions or their interactions were detected by a three-way ANOVA. For the effect of orientation of S+, $F(1, 29) = 2.55$. For the other main effects and the interactions, $F(1, 29) < 1.12$ in every case.

Replaced-stimulus tests. The results of the replaced-stimulus tests are shown in Figure 2. For subjects preexposed to the gray stimulus, more errors were made when S— was replaced than when S+ was replaced, but preexposure to the checkerboard stimulus produced the opposite results. This effect was found for both training stimulus orientations. A three-way ANOVA with orientation of training stimuli, preexposure conditions and replacement of S+ versus S— as main effects was performed. The only significant effect was the interaction of replacement of S+ versus S— and preexposure conditions, $F(1, 29) = 25.54$, $p < .001$. For all other effects and interactions, $F(1, 29) < 0.90$.

The interaction between preexposure conditions and replacement of S+ versus S— was further analyzed by Tukey's LSD method after pooling across training stimulus orientation conditions. This analysis showed that with gray preexposure stimuli, replacement of S— produced significantly more errors than replacement of S+ ($p < .01$) but with checkerboard preexposure stimuli, replacement of S+ produced significantly more errors than replacement of S— ($p < .01$).

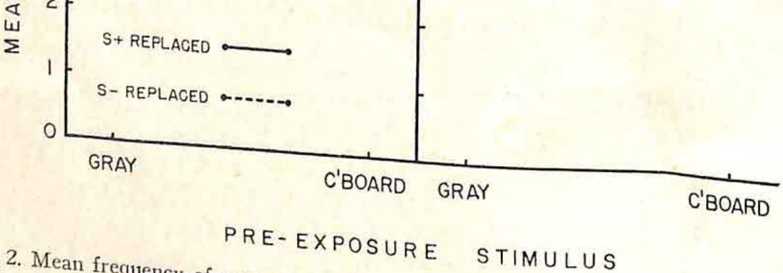

FIGURE 2. Mean frequency of errors made with S+ or S— replaced, plotted separately for preexposure conditions and orientation of training stimuli. (C'board = checkerboard.)

The results are consistent with the hypothesis that manipulation of the novelty of the replacement stimulus affects performance in a replaced-stimulus transfer test. The results obtained in Experiment 1 and by Stevens and Fechter (1968) and Stevens and Wixon (1975) were replicated by animals preexposed to gray panels; that is, relatively more errors were made when S− was replaced as compared to when S+ was replaced. However, animals exposed to the checkerboard panel demonstrated the reverse pattern.

EXPERIMENT 3

Replaced-stimulus transfer tests have been used to assess the relative importance of S+ and S− in discrimination. However, Experiments 1 and 2 demonstrated that manipulation of stimulus complexity and stimulus novelty can critically affect the results of these tests. As noted above, the multiple-stimulus method is another means by which the relative control of S+ and S− has been assessed in simultaneous discrimination. Although this method avoids the use of new stimuli in transfer tests by contrasting acquisition performance, it too is susceptible to confounding by nonassociative factors. With the multiple-stimulus method, results showing faster learning with a constant S− and multiple S+s than with a constant S+ and multiple S−s are ambiguous, for such results can be interpreted as showing relatively greater stimulus control by S− (Mandler, 1970, 1971) or as a result of the incentive properties of changing stimuli.

As an alternative assessment procedure concerning the relative importance of S+ and S− in discrimination learning, the following experiment compared the rates of acquisition between rats trained with either a distinctive S+ or a distinctive S−.

Method

Subjects and apparatus. The subjects were 18 male rats of the Charles River CD strain, about 80 days old. The apparatus was the same as used in Experiment 1. The checkerboard and homogeneous gray stimuli were used.

Procedure. The subjects were randomly assigned to two groups each containing nine rats. The subjects were gentled and adapted to a 23-hr 50-min water deprivation schedule. They were pretrained following the procedure described for Experiment 1, except that all subjects had equal experience with the gray and checkerboard stimuli during pretraining.

Following pretraining one group was trained with the checkerboard as S+ and the gray stimulus as S−, and the other group was trained with the gray as S+ and the checkerboard as S−. Ten trials a day were given until a criterion of nine correct choices within a day was reached.

Results and Discussion

The mean errors to criterion made by the subjects trained with the checkerboard as S+ was 14.9, and by the subjects trained with the checkerboard as S−, 77.4. These means differ significantly; $t(17) = 5.09$, $p < .001$.

That learning was more efficient when a distinctive S+ was used than when a distinctive S− was used is not predicted from any simple assertion that S− is the relatively more important stimulus in discrimination learning. The results are consistent with the position that stimulus quality either elicits unlearned response tendencies which importantly affect discriminative behavior, or influences the acquisition of approach and avoidance tendencies which similarly affect discriminative behavior. Approach responses (to S+) may be acquired more readily to distinctive stimuli than to less distinctive stimuli, while avoidance responses (to S−) may be less likely to similarly benefit from increased distinctiveness of the stimuli.

Using pigeons and successive operant discrimination, Jenkins and Sainsbury (1970) found that responses directed toward distinctive features that were correlated with reinforcement are acquired more rapidly than responses directed away from distinctive features that were correlated with nonreinforcement. The present results can be viewed as demonstrating a similar greater efficiency of "feature positive" as contrasted to "feature negative" discrimination learning.

CONCLUSIONS

Experiments 1 and 2 were directed at demonstrating that results obtained in studies using replaced-stimulus tests may critically depend on the relative complexity or novelty of the replacement stimuli. Experiment 1 demonstrated that results which would seem to indicate that S− is relatively more important than S+ occurred only when a complex stimulus (checkerboard) was used in the replacement test. Similarly, Experiment 2 demonstrated that results consistent with an associative interpretation stressing the relatively greater importance of S− were obtained only when stimuli to which the animal had not been previously exposed were used as replacement stimuli. In contrast, when stimuli to which the animal had been preexposed were used in a replaced-stimulus test the results were consistent with an associative interpretation stressing the greater importance of S+. Thus, the replaced-stimulus test is a questionable technique when used to assess the relative importance of S+ versus S−. The multiple-stimulus technique has produced results (e.g. Mandler, 1970, 1971) interpreted as showing S− is relatively more important than S+ during acquisition. However, this method may also involve nonassociative confounding. Experiment 3 assessed the relative importance of S+ and S− using a method for which results favoring the greater importance of S− would be less ambiguous. In fact, the results were opposite to those that would have been predicted from an interpretation which stresses the role of S−.

Although the results of Experiment 3 are consistent with several associative interpretations, those interpretations cannot account for the results of Experiments 1 and 2. The one interpretation that consistently accounts for the results of not only the three experiments reported here but previous research using simultaneous discrimination methods (Hall, 1973; Mandler, 1968, 1970, 1971; Stevens & Fechter, 1968; Stevens & Wixon, 1975) is one emphasizing the importance of novel, complex, and distinctive stimuli in eliciting nonassociative approach responses.

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Forgetting of a Discrimination After Intervals of Intermediate Length: The Kamin Effect with Choice Behavior

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Several experiments tested the occurrence of the "Kamin effect" in terms of discrimination (choice) behavior by rats. The effect—defined by a nonmonotonic retention function following aversive conditioning, which includes exaggerated forgetting after intervals of intermediate length (1–6 hr.)—was obtained in terms of choice behavior, with each discrimination task tested (brightness and position) and for each of three types of retention test differing in the nature of the transfer between training and testing. The results of these tests suggest that a deficit in memory retrieval after intermediate-length intervals is the most appropriate explanation for these instances of the Kamin effect, with a likely important role in such memory processing to be assigned the endogenous neurochemical and hormonal changes that accompany and follow a stressful learning experience.

A major division exists between theoretical interpretations of the "Kamin effect," which is the deficit in performance of an incompletely learned avoidance task after retention intervals in the intermediate-length range of 1–6 hr. (Kamin, 1957). In the view of some investigators, this phenomenon is due primarily to ineffective processing of the memory that represents the events of training (Bintz, 1970; Klein & Spear, 1970a, 1970b); others believe it reflects a deficit in performance caused by time-dependent changes in the ability to initiate an active response following shock-induced stress (Anisman, 1975; Barrett, Leith, & Ray, 1971b; Pinel & Mucha, 1973).

To test the role of memory processing in the Kamin effect, Barrett et al. (1971b) used a discriminated-avoidance task involving choice behavior. They reasoned that even though an avoidance response measure of "when" to run might be affected readily by motivational or activity state changes, a choice response measure would not, and so the latter should be a more precise index of memory function. In testing after a retention interval of intermediate length, their results indicated relatively slow avoidance responding without a concomitant decrease in percentage of correct choices—a Kamin effect in terms of latencies but not in terms of choice behavior. A second study yielded similar results (Steranka & Barrett, 1973). The authors concluded that the Kamin effect was a function of activational rather than memorial changes, suggesting that the phenomenon is due to shock-induced inhibition that is incompatible with the acquisition of an active-avoidance response.

Contrary to the performance-inhibition interpretation, Klein and Spear (1970a, 1970b) used a paradigm in which rats tested at intermediate-length intervals actually showed more active responding than those tested at shorter or longer retention intervals. In these experiments, rats originally trained to perform a passive-avoidance task were tested 0, 1, 4, or 24 hr. later on a one-way active-avoidance task. Parameters were arranged to ensure that negative transfer occurred between these tasks if the latter immediately followed the former. Rats tested after either the 1- or 4-hr. intervals learned

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the new task faster than those tested a few minutes or 24 hr. after original learning. These authors suggested that at the extreme intervals (0 and 24 hr.), the memory of the original conflicting "no-go" task interfered with the learning of the new "go" task. Rats tested at the intermediate (1- or 4-hr.) intervals showed less negative transfer, learning the active-avoidance task more rapidly and thus exhibiting more activity than those tested at the extreme intervals. But because the intermediate-interval animals clearly showed the poorer retention—indeed, these rats behaved in a manner similar to that of naive animals—the conditions for defining the Kamin effect were satisfied. This retention deficit was attributed tentatively to a deficit in processing (retrieval) of the memory that represented the prior, conflicting learning.

It was proposed later that this retention loss at intermediate length intervals might be produced by a mechanism similar to that of drug dissociation or state-dependent retention (Spear, Klein, & Riley, 1971). In this view, the internal hormonal and neurotransmitter responses to a new stress 1–6 hr. following a stressful event are dissimilar to those that exist during original stress (i.e., training), those prevailing immediately thereafter (0-hr. intervals), or those produced by new stress after a period of recovery (i.e., 24 hr. later). The physiological states at training and during retention tests 0 hr. and 24 hr. later may be quite similar in nature and might include interroceptive stimuli, which, if noticed by an animal during a learning episode, could function at a later time as cues to facilitate retrieval of the memory of that episode. The dissimilarity between such state-produced cues at training and at a test following an intermediate-length interval would not permit such a benefit for memory retrieval.

This memory-retrieval interpretation of the Kamin effect appears capable of explaining those cases in which more active responding is seen at intermediate intervals (Bintz, 1970; Bintz, Braud, & Brown, 1970; Klein & Spear, 1970a, 1970b; Pinel & Cooper, 1966; Ross, 1975; Singh, Sakellaris, & Brush, 1971) as well as the more typical finding of less active responding. Such an interpretation has been suggested in various forms by several authors (Bintz et al., 1970; Brush, 1971; Klein, 1972; Spear, 1971, 1973). However, the failure to find a Kamin effect when retention of discrimination learning was measured in terms of choices, in spite of the simultaneous occurrence of the effect in terms of response latencies (Barrett et al., 1971b; Steranka & Barrett, 1973), would suggest a performance rather than a memorial interpretation of the phenomenon.

The lack of a Kamin effect in choice behavior has been cited often in the literature as perhaps the single strongest objection to a memorial interpretation of the phenomenon (Anisman, 1973, 1975; Barrett, Leith, & Ray, 1973; Caul & Barrett, 1973; Pinel & Mucha, 1973). However, this objection is based on negative results from two studies using nearly identical procedures (Barrett et al., 1971b; Steranka & Barrett, 1973). The Kamin effect is not always easy to reproduce (Anderson, Johnson, Schwendiman, & Dunford, 1966) and has been observed to be "extremely sensitive to . . . seemingly minor procedural variations" (Barrett, Leith, & Ray, 1971a, p. 664). For example, the procedure of Barrett and his associates uses a criterion that limits degree of avoidance-response learning but does not limit degree of choice-response learning, leaving open the possibility that the choice response may be overlearned and rendered less sensitive to the effects of whatever mechanism is responsible for the phenomenon.

A related point is the frequent finding of a difference in the sensitivity of choice and latency measures to the effects of a wide spectrum of variables. For example, the effect of a shift in conditions of reinforcement, while easily detected in terms of response latencies, was not observed in terms of the animals' choices (e.g., Logan, 1968; Spear, 1964; Spear & Spitzner, 1966, 1968, 1969) until a more sensitive technique was devised (Eisenberger, Frank, & Park, 1975). Variation in retention interval, in particular, has been shown to influence response latencies more readily than choice behavior (Hill, Cotton, Spear, & Duncan, 1969).

Another possible reason for failure to find the Kamin effect concerns the testing situation. "Reminder" cues given prior to testing—such as noncontingent footshock (Klein & Spear, 1970b) or simple reexposure to the apparatus (Anisman & Waller, 1971)—tend to decrease the performance decrement found at intermediate-length intervals (Klein, 1972; Spear, 1973). A lengthy relearning test involving footshock during the early trials (as required in the studies by Barrett and his colleagues) would provide such cues and could mask differences that otherwise might be observable during only the early part of testing. The importance of the early test trials has been mentioned previously (Denny & Ditchman, 1962).

The present experiments used escape rather than avoidance contingencies to exclude variance due to the absence versus presence of footshock late in training and thus to emphasize learning criteria based solely on choice behavior. Retention tests were evaluated with special attention to possible differences in choice behavior during the early portions of the testing session before external situational cues could produce a significant reactivation effect.

Two types of T-maze discrimination tasks were used in these experiments. The first was a brightness discrimination task similar to that used by Barrett and his associates but representing a less complex learning task in that the subject is not required to learn to avoid, to withhold responding during the intertrial interval, or to return to a compartment in which it previously had been shocked. This minimized the number of trials necessary for training.

The second task, which also had this property, was based on a position discrimination so that the generality of the results observed in the brightness-discrimination task might be assessed. Although experiments involving brightness and position discriminations were interspersed in actual chronological sequence, for clarity of exposition the series involving brightness-discrimination learning is presented first followed by the series involving the position-discrimination task.

The present experiments provide evidence that the Kamin effect can occur in terms of choice behavior when the task requires discrimination of either brightness or position. With various training and testing procedures, the results lead to the conclusion that the effect, rather than being a simple performance deficit, is due, at least in part, to deficits in the ability to retrieve the critical memory at intermediate intervals following a stressful learning experience.

EXPERIMENT 1

This experiment was designed to determine whether a Kamin effect in terms of choice behavior would occur following nonspecific stress or if prior acquisition of the memory manifested during testing was necessary for the effect. Only the latter would be relevant to a memorial interpretation. A brightness-discrimination escape task in a manually operated T-maze was chosen because it was conceptually similar to the task in which the effect had failed to occur (brightness-discrimination avoidance in an automated Y-maze; Barrett et al., 1971b) but differed appreciably in task complexity.

Three different conditions were used in this experiment, each involving tests of separate groups of rats after short, intermediate, or long retention intervals. In the basic condition, rats simply were trained on the brightness discrimination task (brighter side correct) and tested on a relearning task at various time intervals thereafter. A second set of animals received only equivalent inescapable shock—no discrimination training—prior to being given the same "retention" test as animals in the relearning condition (i.e., brighter side correct). Results with these animals should determine whether shock stress alone could produce performance differences at various retest intervals.

For the third set of animals, the training conditions were arranged to induce negative transfer on an immediate test, that is, poorer acquisition of the conflicting task used for testing. With such a test, an *inverted*-U-shaped performance function is expected by a memorial interpretation, with the best reversal performance (poorest retention) being seen at intermediate intervals, when retrieval of the memory of prior conflicting learning is presumed deficient. The alternative view would expect relatively poor test performance at intermediate-length intervals regardless of the relationship between training and testing tasks.

Method

Subjects and apparatus. The subjects were 90 experimentally naive male albino rats from the Rutgers colony. These rats are first-generation descendents of Sprague-Dawley rats originally obtained from Sprague-Dawley, Inc., Madison, Wisconsin. The subjects were housed in individual cages and provided with ad-lib food and water throughout the experiment. They were 60–90 days old at the start of the experiment.

The apparatus was a T-maze (stem, 36 cm long × 11 cm wide × 13 cm high; each arm, 17 cm long × 10 cm wide × 13 cm high) made of Plexiglas (painted black) mounted above a grid floor that was electrified (1 mA) via a scrambled shock source (Lafayette shock generator Model E1064GS). The stem, choice section, and arms were separated by clear Plexiglas guillotine doors. The light reflected on the back wall of the maze (opposite the stem) from two 7-W incandescent lamps located under both arms of the maze provided the discriminative stimuli on which the task was based.

Procedure. Each rat was hand carried to the experimental chamber, placed in a holding cage for 30 sec prior to the first training trial, then placed in the stem of the T-maze. Shock was initiated immediately. For animals in the relearning- and reversal-test conditions, no door separated the animal from the choice point, but a clear Plexiglas door did block entrance into the darkened, "incorrect" arm of the maze. Thus, the rat was forced to run to the correct, illuminated arm, at which point shock was terminated. The technique of using a forced- rather than free-choice training procedure was suggested by Moot, Overby, and Bolles (1974) as a means for inducing rapid learning of a brightness discrimination. Following entry into the correct arm, the rat remained there for 5 sec and then was removed to the holding cage for a 15-sec intertrial interval. Twenty-five such forced trials were given during training, with the location of the brighter arm determined by the following schedule: LLRRLRRRLLRLLRLRLRRLLRLRR.

The animals receiving inescapable shock were treated similarly except that a clear Plexiglas door separated the rat from the choice point. For these animals, the duration of shock for each of 25 trials was determined by the median escape latencies of animals given the forced-trial training. Again, a 5-sec delay followed the termination of shock, after which the rat was placed in the holding cage for the 15-sec intertrial interval. The arms of the maze, which were visible through the Plexiglas gate, continued to be shifted in illumination in accordance with the above schedule.

Testing for all animals consisted of 25 trials with procedures identical to those of training, except that a different schedule was used (RLRRLRLL-RLRLLRRLRRRLLRLLR), and the gates to both arms were now open so that a free choice was available to the rat. If the rat entered the darkened arm, shock remained on until it entered the correct arm. Nine additional naive rats were not given prior footshocks but were tested along with those in the inescapable-shock condition. Animals in the reversal condition were tested in an identical fashion, except that the darkened arm was now correct and led to escape from shock, whereas the illuminated arm was incorrect and did not.

Each group representing one of the major treatment conditions (relearning; inescapable shock; reversal) was divided by random assignment into three smaller groups of nine animals each, to be tested either 3 min., 1 hr., or 24 hr. following training. Because the major treatment conditions actually were run during different time periods as independent experiments, it was necessary that the results be analyzed separately, with retention interval as the major variable of interest.

Results and Discussion

The results of Experiment 1 indicated that the Kamin effect can occur in terms of choice behavior. Furthermore, the shape of the retention function was determined by the transfer relationship between the training and testing task: With relearning (positive transfer) as the retention test, a performance decrement was found at the intermediate retention interval; with discrimination reversal (negative transfer) as the test, the greatest decrements occurred at the 3-min. and 24-hr. intervals. Animals previously given inescapable shock rather than discrimination training (negligible transfer, as evidenced by lack of a difference between shocked and nonshocked control subjects) did not differ in performance as a function of retention interval. Figure 1 shows these results in terms of the number of correct choices made during the first 10 trials of the retention test. Figure 2 shows the results of all 25 test trials.

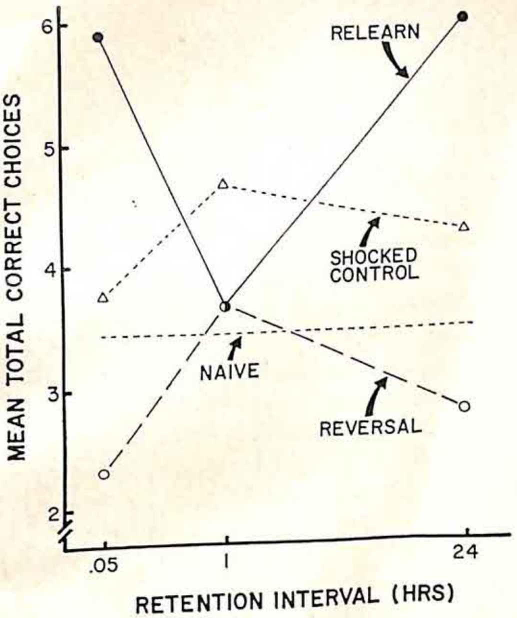

FIGURE 1. Mean total correct choices for the first 10 test trials in Experiment 1. (A correct choice is defined in terms of the testing task. For relearning, shock control, and naive groups, the correct alternative during original learning—for the relearning group—also was correct during testing. For the reversal group the incorrect alternative during original learning was correct during testing.)

For animals tested in terms of relearning performance, analysis of variance by planned orthogonal comparisons (Keppel, 1973) indicated that although animals given 3-min. and 24-hr. retention intervals did not differ significantly, they chose the correct alternative significantly more often than did those animals tested after the intermediate (1-hr.) interval. This was seen in terms of correct choices made during both the first 10 test trials, $F(1, 24) = 9.49$, $p < .01$, and all 25 test trials, $F(1, 24) = 10.71$, $p < .005$. These results indicate that the Kamin effect can occur with a choice behavior measure of retention.

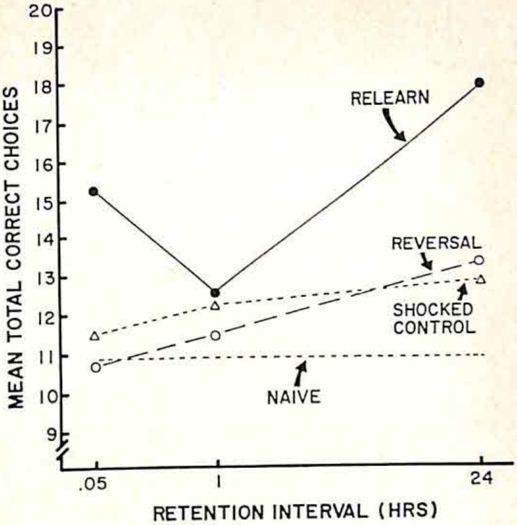

FIGURE 2. Mean total correct choices for all 25 test trials in Experiment 1. (A correct choice is defined in terms of the testing task. For relearning, shock control, and naive groups the correct alternative during original learning—for relearning group—also was correct during testing. For the reversal group the incorrect alternative during original learning was correct during testing.)

As for the inescapable shock condition, no significant differences were found as a function of retention interval. The combined 3-min. and 24-hr. groups did not differ significantly from the intermediate-interval group in terms of the first 10 test trials, $F(1, 24) = 2.36$, $p > .10$, or all 25 test trials, $F(1, 24) < 1.0$. For these subjects it should be noted that although performance at the 1-hr. interval was not significantly better than at 3 min. or 24 hr., the direction of this effect was directly opposite the prediction of a performance-decrement hypothesis. Test performance was uniformly poor for all animals in these conditions, approximating that of animals tested at the 1-hr. interval in the "relearning" treatment condition and did not differ significantly from that of the naive animals that had received no prior treatment ($F < 1$). It appears that the choice response is a measure that is not greatly affected by prior shock stress alone. Retention differences observed following the relearning and reversal treatments therefore may be attributed to deficits in memory processing rather than to activity or arousal deficits per se.

In the reversal condition, an inverted-U-shaped retention function was seen in the first 10 testing trials: The 3-min. and 24-hr. groups made significantly more incorrect choices (indicating better retention of prior conflicting learning) than did the 1-hr.

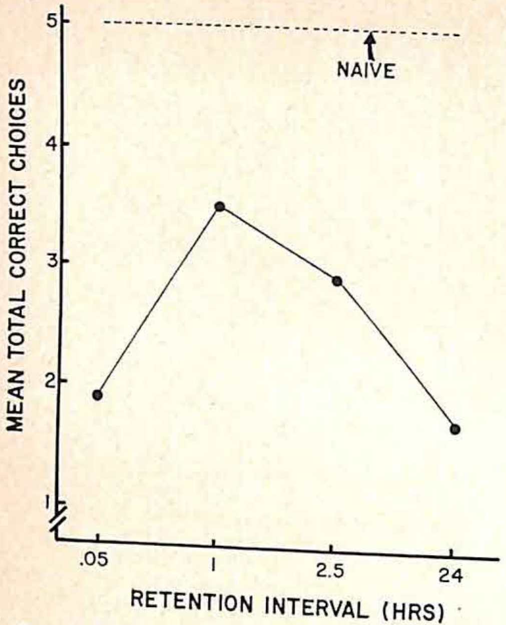

FIGURE 3. Mean total correct choices for the first 10 test trials in Experiment 2. (A correct choice is defined in terms of the testing task. The test involved discrimination reversal—brighter alternative originally was correct; darker alternative is correct during the test. Therefore, better retention is indicated by *fewer* correct choices. A group of formerly naive animals trained on this testing task—darker alternative correct—is shown for comparison.)

group, $F(1, 24) = 5.63$, $p < .05$. This is as predicted by a memory retrieval interpretation of the Kamin effect. This effect was not observed, however, when data from all 25 test trials were analyzed (Figure 2). During the final trials of testing, the 3-min. and 24-hr. groups began to show performance on the new task equal to or better than that of the 1-hr. group. (The mean total correct choices for Trials 11–25 were 9.1, 8.6, 10.2 for the 3-min., 1-hr., and 24-hr. groups, respectively.)

The lack of continuation of the inverted-U-shape function throughout the latter trials of reversal testing could be attributed to several potential causes. These might include (a) reactivation, caused by the early test trials, of the memory of original training, with consequential negative transfer in the later trials for animals in the 1-hr. retention test group; (b) the benefit of positive transfer components (e.g., attending to the appropriate stimulus dimension) among the 3-min. and 24-hr. groups as they learn the reversal task; or (c) the consequences predicted by the performance interpretation of the Kamin effect, which should cause any task to be learned more poorly at intermediate retention intervals, regardless of the transfer relationship between the tasks used for training and testing. The last alternative would imply that the inverted-U-shaped function observed in terms of Reversal Trials 1–10 was perhaps a chance finding and the (statistically nonsignificant) U-shaped function observed in Trials 11–25 is a more accurate representation of behavior in this paradigm. To confirm and expand the results of Experiment 1, a replication experiment was conducted, including some additional conditions.

EXPERIMENT 2

This experiment was designed basically as a replication of the reversal condition of Experiment 1. Aside from an increase in sample size and the use of an additional (2.5-hr.) intermediate-length test interval, the only notable difference between the two experiments was in minor modification of the apparatus.

Method

Subjects and apparatus. Subjects were 24 naive male and 24 naive female rats from the Rutgers colony, 55–70 days old at the time of training. Males and females were divided equally among the four groups, which were tested after different retention intervals.

The apparatus was the same as that used in Experiment 1, except that a wedge-shaped piece of Plexiglas, painted white, was inserted at the choice point attached to the wall opposite the stem. As the subject approached the choice point through the stem, it faced the apex of the wedge. Light from the illuminated arm of the maze was reflected on the side of the wedge closest to that arm, thus providing an additional cue for discriminating the brighter arm from the darker arm.

Procedure. The procedure was identical to that used for the reversal condition in Experiment 1, except that an additional (2.5-hr.) intermediate-length interval also was investigated. An additional group of six male and six female rats (similar in description to those above but run at a later date), received no prior training but were exposed to the same testing procedures (dark side correct) as the trained rats. This group provided a baseline of test performance by naive animals.

Results and Discussion

The result predicted by a memory-retrieval hypothesis, that is, an inverted-U-shaped retention function, was found in terms of the number of correct reversal responses during the test. (Correct reversal responses indicate a lack of negative transfer—that is, forgetting—from original learning.) Figures 3 and 4 show that animals tested after 3 min. or 24 hr. had significantly better retention (more *incorrect* reversal responses) during reversal training than those tested 1 hr. or 2.5 hr. after original learning. This effect was significant in terms of both the first 10 test trials, $F(1, 40) = 17.07$, $p < .001$, and all 25 test trials, $F(1, 40) = 13.88$, $p < .001$. These results confirm that a relative decrement in performance is not always seen after intermediate-length intervals, and in fact, the opposite relationship may occur if training transfers negatively to testing.

EXPERIMENT 3

If the results obtained in the brightness discrimination task also could be observed using a different discrimination task, generality would be increased. Also, the possible role of certain factors unique to the former task could be ruled out. For example, it is possible that the results of Experiments 1 and 2 were influenced by a tendency to prefer darkened areas after intermediate-length intervals following stress, a tendency that might not be present after shorter or longer intervals (although animals tested 1 hr. after inescapable shock alone did not indicate such a preference in Experiment 1 and in fact exhibited a slight initial preference for the illuminated arm; see Figure 1). A position-discrimination task therefore was chosen. With this task, the correct arm of the maze was determined as that opposite the subject's initial choice.

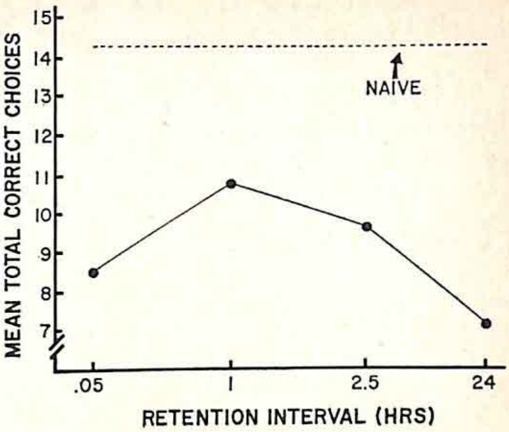

FIGURE 4. Mean total choices for all 25 test trials in Experiment 2. (A correct choice is defined in terms of the testing task. The test involved discrimination reversal—brighter alternative originally was correct; darker alternative is correct during the test. Therefore, better retention is indicated by *fewer* correct choices. A group of formerly naive animals trained on this testing task—darker alternative correct—is shown for comparison.)

Before beginning Experiment 3, it was determined that the present position discrimination is learned quite rapidly: Preliminary tests indicated that with our procedures a mean of only 2.6 trials was required to precede a criterion run of three consecutive correct choices. Because this learning was suspected to be too rapid for stress-induced, internal changes to occur and be noticed by the animal during training, Experiment 3 incorporated the procedure of presenting noncontingent shock just prior to original training. Also, to prevent overlearning that might preclude the Kamin effect, only three training trials were given each animal.

Testing was expected to proceed at least as rapidly as training. Spear (1971, 1973) has noted that the Kamin effect seldom is seen on the first test trial, and probably appears only after response to new stress. With the present task, relearning may be completed for all animals before their internal stress-related responses are fully manifested. To provide the required stress, noncontingent footshock could be given prior to testing, but it also might provide external cues (e.g., experimenter handling, exposure to painful grid shock) that might aid memory retrieval regardless of the accompanying internal state. Therefore, rats in Experiment 3 were placed in an opaque metal container 2 min. prior to testing and were carried, in this container, into the experiment chamber. It was expected that such a novel environment might produce a stress reaction prior to testing (Friedman & Ader, 1967) but would not provide the external stimulus cues accompanying footshock that might mediate memory transfer between different internal states (see Connelly, Connelly, & Epps, 1973; Connelly, Connelly, & Phifer, 1975).

Method

This experiment actually was composed of two independent experiments that included similar training and testing procedures; these are reported as Experiments 3a and 3b.

Subjects and apparatus. Subjects were naive rats from the Rutgers colony, 50–80 days old at time of training. Subjects in Experiment 3a were 36 females, whereas subjects in Experiment 3b were 17 males and 15 females. The 3-min. group was composed of 5 males and 3 females; the other three groups each contained 4 males and 4 females.

The apparatus was the same as that used in Experiment 2. The lights under both arms of the maze were illuminated constantly.

Procedure. All animals were hand carried to the experimental chamber for training and immediately placed in a clear Plexiglas apparatus (30 × 30 × 58 cm high) in which they received four noncontingent footshocks (1-mA for 5 sec, presented 15-sec apart). Each rat remained in this apparatus for 1 min. following the last shock and then was placed in the holding cage for an additional 30 sec. Therefore, the first discrimination training trial did not begin until 2.5 min. following the first shock and 1.5 min. following the final shock.

For each training trial, the rat was removed from the holding cage and placed in the stem of the maze for 5 sec, with a clear Plexiglas door preventing the subject from reaching the choice point. The door was raised and shock (1 mA) initiated simultaneously. Each animal's choice on the first trial was defined as incorrect, as were any subsequent responses made to that arm of the maze. Following an incorrect choice, shock remained on until the subject entered the opposite, "correct" arm. The three training trials were separated by a 15-sec intertrial interval spent in the holding cage. The subject then was returned to its home cage for a retention interval of 3 min., 1 hr., 2.5 hr., or 24 hr.

The opaque metal container containing the subject in its home cage was carried to the experimental chamber 2 min. prior to testing. Testing was similar to training except that the previously correct arm was now incorrect (i.e., a reversal procedure designed to test negative transfer, as in Experiment 2). Testing continued until three consecutive correct (reversal) choices, with a minimum of five trials given to each animal.

Results and Discussion

Groups assigned to different retention intervals did not differ significantly in number of correct choices during training. Analysis indicated little group variation during training in Experiment 3b, $F(3, 28) < 1$, with slightly more variation in Experiment 3a, $F(3, 32) = 2.10$, $.25 > p > .10$. Although the latter differences were not statistically significant, they may have contributed to the anomalous retention effect to be described, found after the 3-min. interval in Experiment 3a.

In each of Experiments 3a and 3b, rats tested after 24 hr. made more incorrect reversal responses during the initial five trials and required more trials to reach the reversal criterion than did those animals tested after 1 or 2.5 hr. (see Figure 5). Both measures therefore indicated that better retention of prior learning occurred among animals tested after a long interval than among those tested after intermediate intervals, in accord with the Kamin effect. This result, however, achieved statistical significance only in terms of trials to criterion in Experiment 3a ($p < .05$, Tukey's honestly significant difference test; Keppel, 1973). In Experiment 3b, the 3-min. and 24-hr. groups made more incorrect responses during the initial five trials than did the combined intermediate interval groups, marginal significance, $F(1, 28) = 4.13$, $p = .052$. Corresponding differences in terms of trials to criterion indicated a similar relationship but were not significant statistically. Each of these relationships is in accord with the Kamin effect, but an unequivocal statement of the effect's occurrence is precluded. In short, largely because of the poor retention observed in the 3-min interval group in Experiment 3a, the results are difficult to interpret.

The discrepancy between Experiments 3a and 3b may be due, in part, to the particular pattern of reinforced (correct) and nonreinforced (incorrect) responses made by individual animals during the original three training trials, which would induce variability in degree of original learning. Experiment 5 was designed to cope with

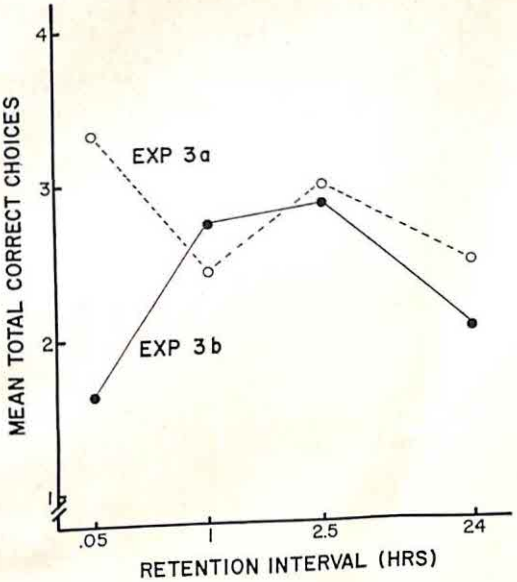

FIGURE 5. Mean total correct choices for the first five test trials in Experiment 3—Replications a and b. (A correct choice is defined in terms of the testing task. The test involved discrimination reversal. Therefore, better retention is indicated by *fewer* correct choices. Corresponding values for the trials to criterion measure for groups tested after .05, 1.0, 2.5, and 24 hr., respectively, were for Experiment 3a, 2.00, 3.56, 2.67, 4.78; for Experiment 3b, 4.88, 3.50, 3.13, 3.88.)

this possibility. The effect could be magnified further by individual variation in speed of acquisition of a new (in this case, reversal) task. This possibility can be avoided by using a test procedure that does not require new learning, as was used in Experiment 4.

EXPERIMENT 4

This experiment used training procedures identical to those of Experiments 3a and 3b and a new test procedure in which an escape response made to either arm of the maze was "correct" and resulted in termination of shock. Because shock termination was not contingent on choice behavior, it was expected that differences in the retention of original training would not be contaminated by differences in ability to learn at the retention test. Experiment 4 actually was composed of three independent experiments (Replications a, b, c), which are reported collectively.

Method

Subjects and apparatus. Subjects were naive rats from the Rutgers colony, 45–75 days of age at time of training. Experiments 4a and 4c each used 36 male subjects, whereas Experiment 4b used 20 males and 20 females, equally distributed among retention-interval groups. The apparatus was the same as that used in Experiment 3.

Procedure. Training and testing were carried out with procedures identical to those of Experiment 3, except that testing consisted of five trials in which a choice response made to either arm was correct, in that it resulted in termination of shock. Retention intervals were 3 min., 1 hr., 2.5 hr., or 24 hr.

Results and Discussion

The results of these three experiments were analyzed separately and also in combination. Because no significant differences were found among experiments (no main effects or interactions), the results of the combined analysis are reported.

Groups assigned different retention intervals did not differ in terms of number of correct choices made during training, $F(3, 104) < 1$, suggesting equivalent degrees of

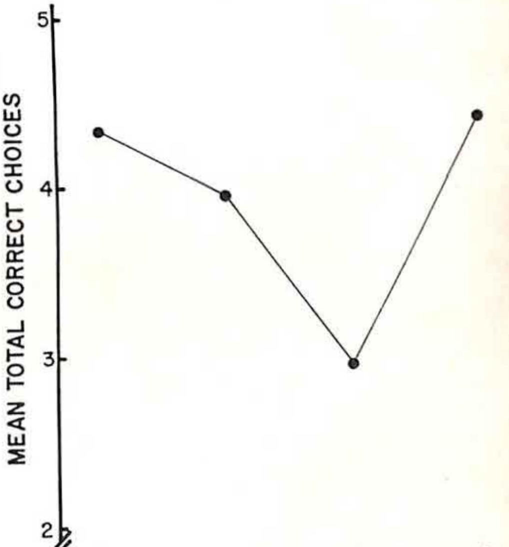

FIGURE 6. Mean total correct choices for the first five test trials in Experiment 4—Replications a, b, and c combined. (A correct choice was defined by the training task since, in this experiment, a response made to either arm during testing was reinforced by termination of shock.)

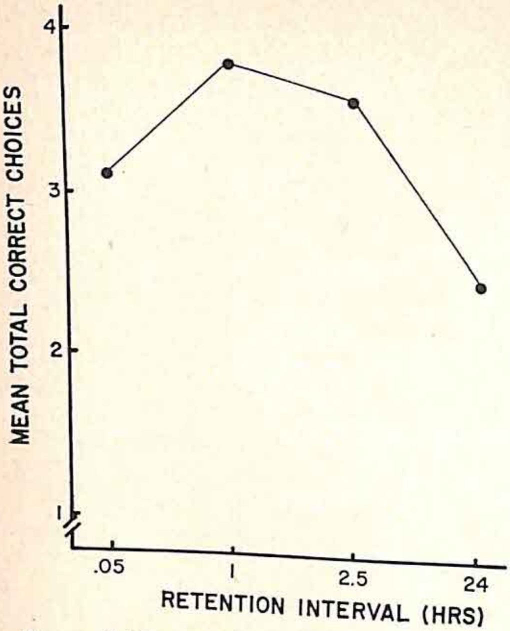

FIGURE 7. Mean total correct choices for the first five test trials in Experiment 5. (A correct choice is defined in terms of the testing task. The test involved discrimination reversal. Therefore, better retention is indicated by *fewer* correct choices. Corresponding values for the trials to criterion measure for groups tested after .05, 1.0, 2.5, and 24 hr., respectively, were 2.11, 1.33, 1.78, 3.56.)

original learning. During testing, the Kamin effect occurred and was verified statistically (see Figure 6). Retention interval was a significant source of variance, $F(3, 96) = 4.50$, $p < .01$. Planned comparisons ($df = 1, 96$) indicated the following: (a) The 3-min. and 24-hr. groups did not differ ($F < 1$); (b) these combined groups (3 min. and 24 hr.) had significantly better retention (more responses made to the formerly correct alternative) than the combined 1-hr. and 2.5-hr. groups ($F = 8.90$, $p < .05$); and (c) the 2.5-hr. group had poorer retention than the 1-hr. group ($F = 5.99$, $p < .025$). It is apparent that the Kamin effect can be observed in choice behavior in terms of a position as well as a brightness discrimination if the test measure is made sufficiently sensitive to potential differences in memory processing.

EXPERIMENT 5

Kamin (1963) indicated that training animals to a low criterion of learning results in less variability and a more pronounced U-shaped retention function than if a fixed number of trials are given. The data of Anderson et al. (1966) suggest that under some circumstances training to one correct avoidance may produce the effect, whereas training to two or three consecutive avoidances may produce overlearning and negate the effect. Therefore, our final experiment included training to a criterion of one correct choice.

Method

Subjects and apparatus. Subjects were 36 naive female rats from the Rutgers colony, 70–90 days of age at time of training. The apparatus was the same as that used in Experiments 2–4.

Procedure. The procedure was identical to that of Experiment 3 (tests given 3 min., 1 hr., 2.5 hr., or 24 hr. after training; reversal test to criterion), except for two changes. First, training was given until one correct choice was made, rather than a fixed number of training trials. Second, the preshock presentations were changed to more closely resemble the sequence of shocks received during a typical multitrial training task. Previously, 4 5-sec shocks were given with a 15-sec intershock interval, followed by a 90-sec delay before training; in the present experiment, 10 2-sec shocks were given with a 15-sec intershock interval, followed by a 15-sec delay prior to the first training trial. Thus, time from initiation of preshock to the first training trial (2.5 min.), total duration of shock (20 sec), and intensity of shock (1 mA) remained the same, but time from the final preshock to training was reduced from 90 to 15 sec.

Results and Discussion

Groups assigned to different retention intervals did not differ in terms of trials to criterion during training, suggesting equivalent degrees of original learning, $F(3, 32) < 1$. During testing, the inverted-U-shaped retention function did appear, as predicted by the memory-retrieval hypothesis. Analysis of variance indicated a significant effect of retention interval in terms of both trials to criterion and number of correct reversal choices during the first five test trials (see Figure 7). Planned comparisons indicated that the 3-min. and 24-hr. groups showed significantly better retention of prior conflicting learning (i.e., poorer reversal) than the 1-hr. and 2.5-hr. groups combined on both number of incorrect choices during Trials 1–5, $F(1, 32) = 9.12$, $p < .005$, and for trials to criterion, $F(1, 32) = 5.90$, $p < .025$.

General Discussion

This series of experiments provide ample evidence that the Kamin effect can appear in terms of choice behavior and is not restricted to tasks that use vigor (go, no-go) measures. The effect appeared in both brightness- and position-discrimination tasks, indicating the generality of the phenomenon.

The use of three different testing procedures (relearning, reversal, and a procedure in which choice responses were not differentially reinforced) helped to eliminate the possibility that the Kamin effect is due entirely to simple performance decrements or deficits in learning capacity that occur after intervals of intermediate length following stress. When net positive transfer from original learning was involved (i.e., relearning), test performance was poorest at intermediate-length intervals following training; but when recall of previous training could be expected to produce negative transfer (i.e., reversal), performance on the new task was best at these intervals. On a test in which no new learning was required and the animal's choice behavior could be based only on retention of reinforcement contingencies existing in the past (i.e., no differential reinforcement of choice responses during testing), the Kamin effect occurred nevertheless, suggesting that a deficit in the retrieval of acquired memories—rather than an inability to learn or to perform—is responsible for the phenomenon. Finally, when no specific transfer was expected—when inescapable shock rather than discrimination training preceded testing—the Kamin effect did not appear.

A performance-decrement hypothesis predicts poorer performance after intermediate intervals following shock-induced stress regardless of the specific transfer from training to testing. A memory-retrieval hypothesis predicts that retention performance is a function of net transfer effects between training and testing. The present data agree with the latter hypothesis.

We are sympathetic, nevertheless, with investigators who have reported negative results (Barrett et al., 1971b, Steranka & Barrett, 1973). During the course of the present investigation, several other experiments, performed in an attempt to extend our understanding of the variables responsible for the phenomenon, did not yield a Kamin effect. Such negative results should be noted, although at present they can add little to an analysis of the phenomenon. For example, procedural changes that were intended to increase or decrease degree of original learning frequently resulted in apparent ceiling or floor effects that precluded measurement of the phenomenon. Perhaps, however, these procedural changes also may have altered the amount of original stress, thereby shifting the point of maximal disruption to a time period following training that was not sampled in these studies (see Riley, Dunlap, & Hughes, 1975). What we can assert nevertheless is that throughout all of our experiments on this topic, whenever retention deficits were detected at all they occurred at the intermediate-length intervals. The lone exception among nearly 20 experiments in this series was the anomalous deficit after 3 min. found in Experiment 3a.

Measurement problems alone, however, appear unlikely to have produced the results of the Barrett et al. (1971b) and Steranka and Barrett (1973) studies. Ceiling or floor effects were neither noted nor apparent. Also, the Kamin effect was observed using the avoidance (latency) response measure, suggesting that appropriate retention intervals were examined. The reason for the discrepancy between these and the present experiments is not entirely clear. Several factors may have contributed to the fact that Barrett and his colleagues did not observe the phenomenon in terms of choice behavior such as length of the test session; the differential sensitivity of choice and latency measurements; reactivation effects during their lengthy test sessions; and probably others. The following possibility, however, seems to us particularly worthy of consideration.

While investigating the effects of drug-induced changes on internal state, Leith and Barrett (1975) noted that in their discrimination avoidance task (the same task used in Barrett's earlier experiments), "avoidance responding did not appear to any great degree until the animals in that group were discriminating at nearly perfect levels" (p.

296). They further suggested a differential sensitivity of the two response measures (choice and avoidance), probably due to overlearning of the discrimination response versus incomplete learning of the avoidance response, and they noted that with increased learning a response becomes more resistant to the dissociation normally produced by drug-induced state change (also see Bliss, 1974). A memory-retrieval interpretation of the Kamin effect may be viewed as an empirical extension of drug dissociation, though in this case, based on state changes of an *endogenous* source (see Spear, 1973, 1976; Spear et al., 1971). On this basis, the occurrence of the U-shaped retention function in terms of the avoidance response but not in terms of choice becomes readily understandable. The avoidance response was learned relatively incompletely and was thus subject to the effects of endogenous state changes, whereas the overlearned choice response was insensitive to these changes.

When Memorial and Performance Interpretations Diverge

It seems reasonable to assume that activity or reactivity changes contribute to the eventual outcome of any experiment that investigates behavior following stressful learning situations. Changes in internal concentrations of hormones such as the adrenocorticotropic hormone (ACTH) have been shown capable of producing state-dependent dissociation effects (e.g., Gray, 1975; Pappas & Gray, 1971), but they also have been demonstrated to have a direct effect on performance and perhaps on learning as well (cf. deWied, 1974). Similarly, chemicals that influence the action of neurotransmitters such as acetylcholine and norepinephrine have been shown to have effects on activity and response initiation (Anisman, 1975), as well as on dissociation (Overton, 1966). The relative importance of each of these factors probably will be determined by the nature of the task under investigation.

The apparent regularity with which the Kamin effect appears with shuttle-box avoidance procedures may reflect a combined impact of memorial and performance (e.g., activity or "coping" capacity) deficits. However, the relatively more difficult task of observing the phenomenon when using a choice-response measure may suggest that only memorial deficits have a measurable effect in this task. Furthermore, under circumstances in which performance effects may oppose those produced by deficits in memory processing, the latter often appears to exert the greater influence. This is clear, for example, when poorer retention performance after intermediate intervals is manifested as *more* vigorous behavior (e.g., Bintz, 1970; Bintz et al., 1970; Klein, 1972; Klein & Spear, 1970a, 1970b; Pinel & Cooper, 1966; Ross, 1975).

With reference to task using a choice response measure, increased activity in the cholinergic inhibitory system (see Carlton, 1963)—the mechanism assumed responsible for the response suppression seen after intermediate-length intervals following stress (Anisman, 1975; Anisman & Kokkinidis, 1974)—may be expected to result in *better* discrimination performance. Drugs that facilitate cholinergic activity (e.g., physostigmine) produce better performance in a brightness discrimination task by lowering the rate of incorrect or inappropriate responding (to S^{Δ}), but the rate of correct or appropriate responding (to S^{D}) is unaffected (Leaton & Kreindler, 1972). Drugs that decrease cholinergic activity (e.g., atropine, scopolamine) lead to poorer discrimination performance by increasing incorrect response rates (during S^{Δ}), again leaving correct-response rates (during S^{D}) unaffected (Johnson, Anderson, Khalili, & Beatty, 1972). Scopolamine has in fact been shown to impair discrimination performance in a Y-maze avoidance task (Leith & Barrett, 1975). If cholinergic activity is greatest at intermediate retention intervals, and if the direct performance effect of increased cholinergic activity is enhanced discrimination performance, then an interpretation of the present data in terms of performance effects alone appears to be incomplete. The impaired discrimination performance observed at intermediate retention intervals in positive transfer situations (Experiments 1 and 4) must be due to some other mechanism—

perhaps to the potential state-dependent, dissociative consequences of changes in the levels of internal neurochemical activity.

It is not a matter of great import to determine which mechanism—memorial or performance—exerts the larger influence on the Kamin effect in the wider variety of circumstances. It is essential, however, that further investigation in this area consider both memorial and performance factors and separate their relative influences on behavior within a particular task. To do this, more precise knowledge of the internal physiological state of the organism during training and testing must be acquired so that a comparison of these internal states can be undertaken regarding their dissociative and activational effects. A further requirement, in concert with this knowledge, is more exact control of environmental details of procedure (such as prior handling or familiarization to a novel training environment), since these factors may determine, to perhaps a surprising extent, the internal state of an animal during training or testing (Friedman & Ader, 1967). For example, in the position-discrimination experiments, the procedures used to induce stress prior to training (noncontingent shock) or testing (confinement) may have been essential requirements for finding the Kamin effect. Thorough investigations of this sort would be a prodigious undertaking but could lead to a better understanding of the influence of neurohormonal changes on behavior, offer a model of how state-dependent dissociation might operate as a consequence of changes in endogenous chemicals, and provide insight into the mechanisms with which memories are formed and by which they are retrieved.

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Attenuation of the Latent Inhibition Effect by Prior Exposure to Another Stimulus

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Repeated, isolated presentations of a stimulus typically reduce the subsequent ability of that cue to become an effective conditioned stimulus. This phenomenon is generally referred to as the latent inhibition effect. In this article four experiments are reported that lead to the conclusion that the latent inhibition effect itself can be reduced if repeated presentations of another stimulus are administered *prior* to preexposing the to-be-conditioned stimulus. The theoretical implications of this finding are discussed in terms of two alternative conceptions. One approached the phenomenon as representing a memory retrieval failure, the other considered the effect to be an instance of a failure in the memory storage processes that normally underlie the latent inhibition effect.

If a stimulus has been presented repeatedly prior to being paired with a reinforcer it is retarded, relative to a novel cue, in becoming a signal for either a Pavlovian conditioned reaction or an instrumental response. This phenomenon has received considerable attention and is commonly referred to as the latent inhibition effect (e.g., Lubow, 1973; Lubow & Moore, 1959).

Lubow's (1973) review of this literature indicates that researchers have isolated a number of variables that systematically influence the latent inhibition effect. For example, the degree to which a cue will be latently inhibited has been found to increase with the number of presentations prior to conditioning (e.g., Domjan & Siegel, 1971; Lantz, 1973, Exp. 1; Lubow, 1965; Siegel, 1969) and to increase as the duration of the interval separating stimulus presentations is increased (e.g., Lantz, 1973, Exp. 2). Very little is known, however, about how prior exposure to more than one stimulus event influences the latent inhibition effect.

Recently, Mackintosh (1973) reported an observation which suggests that the number of isolatable stimulus events to which a subject is preexposed may be an important determiner of latent inhibition. The experiment he reported was one which included three groups: the two standard conditions for demonstrating a latent inhibition effect, that is, a preexposure group and a group for which the conditioned stimulus was novel on its first pairing with the unconditioned stimulus (US), and a third group, which was preexposed to the stimulus subsequently used in conditioning, but which, on all preexposure trials, received this stimulus in conjunction with another novel cue. Mackintosh reported the usual latent inhibition effect and, in addition, observed that the cue preexposed in conjunction with another novel event was significantly faster to condition than the standard latent inhibition condition. Thus, the latent inhibition of one cue was attenuated by the simultaneous presence of another novel stimulus during the preexposure phase. The experiments to be reported began from the attempt to investigate this effect further.

Experiment 1

In the critical condition in which Mackintosh (1973) observed the reduced latent inhibition effect, subjects were preexposed to a two-stimulus compound, S_1S_2, and con-

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ditioned only to one cue, S_2. This condition was repeated in the present experiment along with a condition in which subjects received experience with the nonconditioned element of the compound, S_1, prior to S_1S_2 presentations. A number of researchers have suggested that stimulus preexposure retards conditioning because it reduces in some way a cue's saliency or attention-capturing properties (e.g., Carlton & Vogel, 1967; Mackintosh, 1975; Reiss & Wagner, 1972; Rescorla, 1971). If this is the case then one might expect that prior exposure to S_1 would attenuate its influence on the latent inhibition of S_2: In other words, S_2 might be more latently inhibited when it is preexposed in conjunction with a preexposed S_1 than when S_1 is novel.

The complete experimental design included four conditions: (a) Group N, a standard latent inhibition group preexposed only to one stimulus, a noise; (b) Group NL, preexposed to a compound stimulus consisting of a noise and a light; (c) Group L-NL, preexposed first to the light alone then to noise-light compound presentations; and (d) Group C, the control condition which was not preexposed to either the light or noise. All subjects were subsequently conditioned to the noise stimulus by pairing it with a shock US in a conditioned emotional response situation (Estes & Skinner, 1941).

Method

Subjects. Subjects were 32 male Sprague-Dawley rats (325–433 g) purchased from the Perfection Breeding Company, Douglasville, Pennsylvania. Two animals died during the course of the experiment so that data are reported from 30 animals. Rats were housed individually in a continuously illuminated room.

Apparatus. The experimental chambers consisted of eight standard operant chambers (19.5 cm high × 29 cm × 23.5 cm). Centered on the aluminum front wall of each box was a recessed food cup and just above it a bar. Mounted just behind holes in the front wall, to the left and below the food cup, was a 4 in. (10.16 cm) speaker which could be connected to a Grason-Stadler noise generator and an Ashman Electronics Ltd. eight-channel audiosplitter. The remaining rear wall of the chamber was constructed of aluminum while the ceiling and two sides of the box were made of clear acrylic plastic. When energized a 7.5-W bulb located above the ceiling of each chamber provided diffuse illumination within the box through a sheet of white translucent plastic placed on the ceiling. The floor of the operant chamber was composed of 2 mm stainless-steel rods spaced 12 mm apart and connected to a Grason-Stadler shock generator (E1064GS).

Operant chambers were housed within light- and sound-resistant chests. Thus, the normal condition of each chamber was complete darkness. Exhaust fans for each chest provided an ambient noise level within the chests of approximately 60 dB (C). Sound intensities were measured within the chamber by a General Radio Sound-Level Meter (Type 1551-C). Experimental events were controlled automatically by electromechanical equipment located in an adjacent room.

Procedure. Following an approximately 7-day period during which animals were reduced to 75% of their normal body weight, bar press training began. In the first session, food pellets (45-mg Noyes pellets) were initially delivered on a variable-time 2.5-min. schedule. In addition, each bar press yielded a food pellet. Once an animal had emitted 10 responses, pellets were no longer freely delivered, but each bar press response produced one pellet. After a rat had made a total of 100 responses, it was removed from the chamber. Animals which failed to meet this criterion within about 60 min. were removed from the Skinner box and several hours later given a second opportunity to bar press under the same conditions as described above. No animal failed to bar press given a second session. On the following day, (and for the remainder of the experiment) the experimental session was 2 hr. long, and each rat received bar press training under a 2.5-min. variable interval (VI) food reinforcement schedule.

After 3 days of VI training the animals were assigned to the various experimental conditions such that the mean and median bar press rate of the groups were approximately equal as indicated by the third day of VI training. Groups N and NL each consisted of eight subjects, and there were seven subjects in Groups C and L-NL. The stimulus preexposure phase began on the 4th day of VI training. On each of the next 4 days of VI training, the animals in Group L-NL experienced 6 3-min. presentations of the 7.5-W light for a total of 24 light preexposure trials. The remaining three groups continued to receive VI training on these 4 days. On each of the next 2 days of the experiment subjects in Group N experienced 6 presentations of a 3-min. 80-dB noise stimulus, whereas Groups NL and L-NL each received 6 presentations of a 3-min. noise-light compound stimulus event. These stimulus presentations also were imposed on the ongoing bar press behavior of the animals. On these 2 days, the animals in Group C received only bar press training. During each session of stimulus preexposure, the stimulus was programmed to occur

15, 30, 55, 70, 100, and 115 min. after the animal was placed in the experimental chamber.

Conditioning to the noise CS began on the day following the stimulus preexposure treatment. Four conditioning trials consisting of the 3-min. noise terminating in a 1.6-mA, .5-sec scrambled shock delivered to the animal's feet via the grid floor of the chamber were administered. Such trials were given 23, 52, 72, and 108 min. after the animals were placed in the conditioning chamber.

The measure of interest during conditioning was amount of suppression of bar press responding produced by the noise stimulus. Suppression ratios (Annau & Kamin, 1961) were formed according to the expression B/A+B with B representing number of bar presses during the CS and A number of bar presses during a 3-min. baseline period immediately prior to the CS. Thus, a ratio of 0 indicates complete suppression of bar press responding during the CS (good conditioning) and a value of .50 indicates comparable rates of responding during baseline and CS periods (little or no conditioning). Such suppression ratios were calculated only after inspection of the data indicated that there were no reliable differences among the conditions in the number of responses made during the pre-CS period. This was the case for all of the comparisons discussed in the several studies reported in this article.

Results and Discussion

Noise preexposure. The only notable effect observed during the prexposure treatments occurred in the noise preexposure phase. The two conditions that experienced the noise in conjunction with the light, Groups NL and L-NL, evidenced more unconditioned suppression than did Group N, which only experienced the noise. This effect was most prominent on the first trial of noise preexposure, where the median suppression ratio was .25, .30, and .46 for Groups NL, L-NL, and N, respectively. It should be noted that in spite of having been preexposed to the light, the subjects in Group L-NL suppressed as much to the light-noise compound as those in Group NL. Orthogonal comparisons supported these observations. Groups NL and L-NL differed significantly from Group N, $F(1, 15) = 8.67$, $p < .025$, but did not differ from each other, $F < 1$ (data from two subjects in Groups NL and N and one subject in Group L-NL were lost because of experimental error).

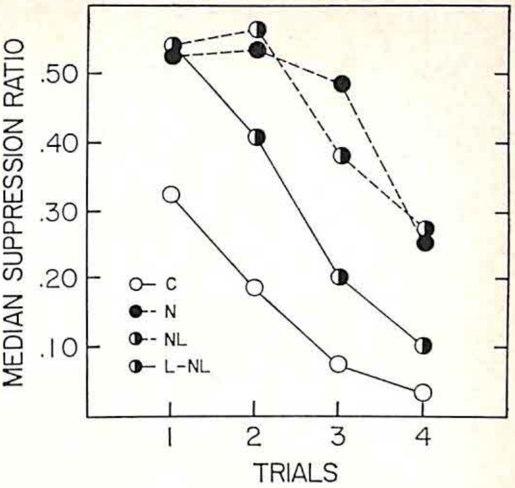

FIGURE 1. Median suppression ratios for each condition as a function of training trials (Experiment 1).

Conditioning data. Figure 1 presents the median suppression ratios for each group as a function of conditioning trials. The first point to be observed is that a substantial latent inhibition effect was obtained. Note that Group N displayed much less suppression to the noise than did Group C. It should also be seen that Group C was more suppressed to the noise on the first trial than were any of the other conditions. Thus, a major component of the stimulus preexposure effect in the present situation is a reduction in the unconditioned suppression properties of the noise. Of primary concern to the purposes of this experiment is the performance of the three groups that received preexposure to the noise. Figure 1 reveals that Groups NL and N showed equivalent performance throughout conditioning. In contrast, Group L-NL appeared to condition more rapidly than did either Group N or NL.

Two separate analyses of variance were performed on these data. The first analysis examined the data for Trial 1 and revealed a significant F ratio, $F(3, 26) = 16.14$, $p < .001$. It is quite clear that this analysis reflects the difference between Group C and the other three conditions. The second analysis, performed on the Trial 2 data, also

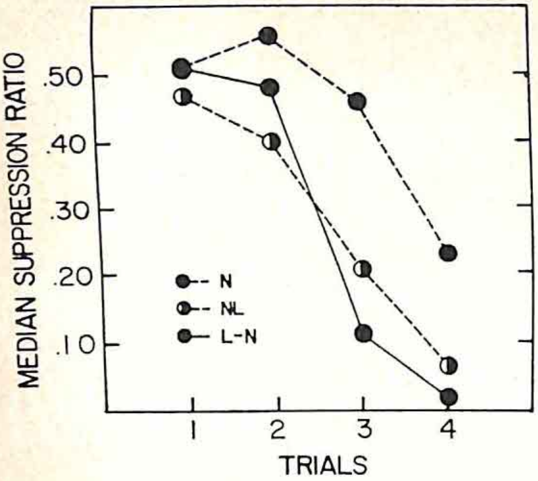

FIGURE 2. Median suppression ratios for each condition as a function of training trials (Experiment 2).

was significant, $F(3, 25) = 25.21$, $p < .001$.[1] Subsequent *t*-test comparisons using the error term of the analysis of variance indicated that (a) Group C differed from Group N, $t(25) = 7.50$, $p < .001$, from Group NL, $t(25) = 7.56$, $p < .001$, and from Group L-NL, $t(25) = 5.15$, $p < .001$, and (b) Group L-NL differed from both Group N, $t(25) = 2.35$, $p < .05$, and from Group NL, $t(25) = 2.41$, $p < .025$.

Recall that Mackintosh (1973) found that the amount of latent inhibition to a cue was attenuated when it was preexposed in conjunction with another novel stimulus. For reasons that are not immediately apparent, the Mackintosh (1973) observation was not replicated in the present study, as Groups N and NL did not differ.

The behavior of Group L-LN also deviated from what was anticipated. As previously noted, it was expected that prior presentations of the light would reduce the extent to which it would interfere with the development of latent inhibition to the noise. Apparently, this manipulation, if anything, produced just the opposite effect.

EXPERIMENT 2

In the second experiment an attempt was made to clarify further the influence of prior exposure of the light stimulus on subsequent development of latent inhibition to the noise. In the first study, the previously exposed light stimulus was also present during presentations of the noise. The major purpose of the second study was to determine if prior exposure to the light would attenuate the development of latent inhibition to the noise even if the light were not present during the presentations of the noise. A secondary purpose of this experiment was to attempt again to replicate the effect reported by Mackintosh (1973). Thus, the second experiment included three conditions: (a) Group N, which received preexposure to the noise event, (b) Group NL, which received preexposure to the noise-light compound, and (c) Group L-N, which first received a sequence of light stimulus presentations followed subsequently by preexposure to the noise stimulus. Again, all subjects were conditioned to the noise in a conditioned emotional response situation.

Method

Subjects and apparatus. The subjects were 24 male Sprague-Dawley rats (330–394 g) maintained at 75% of their normal body weight. The apparatus and CS-US parameters were identical to those used in Experiment 1.

Procedure. The training procedures from Experiment 1 were also used in this study; Groups N and NL thus were treated identically to Groups N and NL of Experiment 1. The new condition of this experiment, Group L-N, was treated exactly as Group L-NL of Experiment 1 with the important exception that the light stimulus was not present when noise preexposures were administered. Nor did these animals receive any further presentations of the light beyond the initial 24. There were eight subjects in each condition.

Results and Discussion

Noise preexposure. As was the case in the first experiment, the only notable effect that was observed during the preexposure phase occurred during the noise preexposure trials. The subjects suppressed more to the noise-light compound than to the noise alone. This effect was most pronounced on the first trial of noise preexposure in which the median suppression ratio was .47, .46, and .27 for Groups N, L-N, and NL, respectively. Note that the prior exposure to light experienced by subjects in Group L-N had little influence on their unconditioned reaction to the noise. Orthogonal comparisons revealed that Group NL differed from Groups N and L-N, $F(1, 21) = 15.80$, $p < .001$, but that Groups N and L-N did not differ, $F < 1$.

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[1] The data from one subject in Group C was discarded because it failed to make two or more responses during either the pre-CS or CS period. This was also the case for one subject in Group L_8 on Trial 4 of Experiment 3.

Conditioning. Figure 2 presents the median suppression ratios for each of the three groups as a function of conditioning trials. Note first that the groups were very similar on the initial trial of conditioning, but thereafter began to diverge. As was the case in Experiment 1, the animals in Group N displayed very little suppresson to the noise until Trial 4. Both Groups NL and L-N, however, appeared to condition more rapidly than Group N but did not themselves differ.

To evaluate these results statistically, a Group × Trial analysis of variance was performed on these data. This analysis revealed both a reliable main effect of Group, $F(2, 21) = 5.07$, $p < .025$, and a reliable effect of Trial, $F(3, 63) = 54.86$, $p < .001$. The Group × Trial interaction, however, failed to reach statistical significance, $F(6, 63) = 1.79$, $p > .10$. Subsequent t tests indicated that both Groups L-N and NL were reliably different from Group N, $t(21) = 2.69$, $p < .02$ and $t(21) = 3.18$, $p < .01$, respectively.

Two points concerning these data warrant reiteration. First, the results obtained by Mackintosh (1973) were replicated. More latent inhibition was observed when the noise cue was preexposed alone than when it was preexposed in compound with the light CS. The discrepancy between Experiments 1 and 2 on this point is not easily understood since the same treatment was given to Groups N and NL in both experiments. Apparently, the parameters employed in these two studies were less than optimal for generating the effect. Second, and more important, it should be appreciated that exposure to the light prior to noise preexposure attenuated the display of latent inhibition to the noise.

EXPERIMENT 3

That prior presentations of a light stimulus should attenuate the subsequent development of latent inhibition to the noise event is not intuitively obvious. Indeed, on some grounds one might reasonably have expected the opposite outcome. For example, one might have assumed that the light and noise events are in some ways similar and that consequently the effect of preexposing the subjects to the light might generalize to enhance the latent inhibition of the noise event. Since this is not what happened, the observation—that prior presentations of the light attenuated the development of latent inhibition to the noise—might be revealing important but previously unsuspected properties of the processes which mediate latent inhibition.

In order to be confident about the reliability of this effect and to be assured that it was not the outcome of a fortuitous choice of parameters, a third experiment was conducted. In this study, the effect of number of light presentations prior to presentations of the noise stimulus was evaluated. Four groups of subjects were examined, each of which experienced the same number of noise preexposure trials, 12. They were distinguished by whether noise presentations were preceded by 0, 6, 12, or 24 light presentations. The 0 condition corresponds of course to Group N of Experiments 1 and 2. All subjects were finally conditioned to the noise.

Method

Subjects and apparatus. The subjects were 32 male Sprague-Dawley rats (336–424 g) maintained at 75% of their normal body weight. The apparatus and CS-US parameters were identical to those used in Experiments 1 and 2.

Procedure. The same training procedures described in Experiment 1 were used unless otherwise noted. Beginning with the 4th session of VI bar-press training, animals in Group L_{24} were given six presentations of the 3-min. light stimulus during each of four consecutive daily 2-hr. ses-

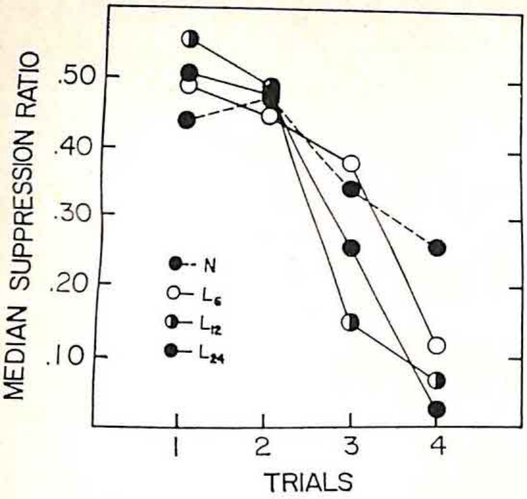

FIGURE 3. Median suppression ratios for each condition as a function of training trials (Experiment 3).

sions. Animals in Group L_{12} were given light presentations during each of two consecutive sessions. For these subjects light presentations began on the 6th session of VI bar-press training. Animals in Group L_6 were given only six light presentations. These occurred during the 7th daily VI bar-press training session. Animals in Group N experienced no light presentations. During the 8th and 9th daily sessions all animals received six presentations of the 3-min. noise stimulus. All animals were given four conditioning trials with the noise serving as the CS during the 10th session.

Results and Discussion

Noise preexposure. The median unconditioned suppression ratios on the first trial of noise preexposure were .35, .34, .40, and .44 for Groups N, L_6, L_{12}, and L_{24}, respectively. Although there appears to be a slight tendency for unconditioned suppression to the noise to decrease as a function of the number of light preexposures, no statistically reliable differences were observed. These data were analyzed by using orthogonal comparisons, Group N versus Groups L_6, L_{12}, and L_{24}; L_6 versus L_{12} and L_{24}; and L_{12} versus L_{24}. In no case was a reliable F ratio obtained, $F(1, 28) \leq 1.68$, $ps > .10$ for all comparisons. Thus, as was the case in Experiment 2, there was no evidence that prior exposure to the light altered the animals' unconditioned responding to the noise.

Conditioning. Figure 3 presents the median suppression ratio for each group on the four trials of conditioning. It can be seen in this figure that by the last trial of conditioning Groups L_6, L_{12}, and L_{24} began to display more suppression to the noise than did Group N. There was also a suggestion that Group L_6 conditioned less well to the noise than Groups L_{12} and L_{24}.

To evaluate these data statistically, a Group × Trial analysis of variance was performed. This analysis yielded both a reliable effect of trial, $F(3, 83) = 65.91$, $p < .001$, and a reliable Group × Trial interaction, $F(9, 83) = 2.40$, $p < .025$. The group effect, however, failed to approach significance, $F < 1$. To assess the Group × Trial interaction further, Group N was compared against each of the other conditions on Trial 4; t tests revealed that Group N differed reliably from both Group L_{12} and L_{24}, $t(27) = 2.50$, $p < .025$, and $t(27) = 2.10$, $p < .05$. The difference between Groups N and L_6, however, was not reliable, $t(27) = 1.31$, $p > .25$.

The important point to appreciate from this experiment is that again it was found that prior presentations of a light stimulus can attenuate the latent inhibition of a subsequently preexposed noise event. This experiment thus replicates the results of Experiment 2. In addition, there was some suggestion that the number of prior light presentations was an important determiner of this effect, as both Groups L_{12} and L_{24} displayed less latent inhibition than Group N, whereas Groups L_6 and N were not significantly different.

EXPERIMENT 4

In the preceding two studies evidence was presented that prior exposure to one stimulus, a light, can attenuate the extent to which subsequent preexposure to a second cue, a noise, produces a latently inhibited noise. In the fourth experiment, the generality of this phenomenon was further examined by determining if prior exposure to the noise stimulus would also attenuate the extent to which subsequent presentations of the light stimulus would latently inhibit the light. In addition, the intensity of the noise stimulus was included as a variable in this study. Thus, five treatment conditions were examined, in which animals were preexposed to a light CS. Four conditions differed by whether the 12 light presentations were preceded by 12 presentations of either a 65-, 70-, 80-, or 90-dB noise stimulus. The fifth group only experienced the 12 light presentations. All animals were subsequently conditioned to the light stimulus.

Method

Subjects and apparatus. The subjects were 40 male Sprague-Dawley rats (380–433 g) maintained at 75% of their normal body weight. The apparatus was identical to that used in Experiments 1, 2, and 3 with the exception that a General Radio Company Sound and Vibration Analyzer (Type 1564-A) was used to evaluate sound intensity. Four levels of sound intensity were calibrated for the noise stimulus, 90 dB, 80 dB, and 70 dB using the "all-pass" bandwidth of the meter; the fourth sound intensity nominally 65 dB was determined by setting the sound intensity 5 dB below that obtained at 70 dB read at several $\frac{1}{3}$-octave frequency bands. The latter procedure was used to avoid unreliable readings due to ambient noise levels near 65 dB.

Procedure. The training procedure used in this study was similar to those of the previous three experiments, except for the following details: On Day 6 and 7 of VI bar-press training, the animals in the four noise preexposure groups received six daily presentations of a 3-min. noise stimulus at either 65, 70, 80, or 90 dB depending on the treatment condition. On Days 8 and 9, all animals were given six daily presentations of the light stimulus. Conditioning to the 3-min. light began on Day 10. Each animal was given four light-shock pairings on this day. On Day 11 all subjects were given four light presentations; however, the shock was presented only on Trial 2. The next 2 days were extinction sessions. Four nonreinforced light presentations were given on each of these 2 days.

Results

Noise preexposure. Unconditioned suppression to the noise during preexposure was systematically related to noise intensity. Subjects suppressed more to the relatively more intense stimulus. The effect was most clearly seen on Trial 1 where the median suppression ratios were .47, .49, .38, and .33 for the animals exposed, respectively, to the 65-, 70-, 80-, and 90-dB noise. Orthogonal comparisons revealed that the two more intense, 90- and 80-dB, stimuli produced more suppression than the two less intense, 65- and 70-dB, stimuli, $F(1, 28) = 20.03$, $p < .001$. The difference between the 80- and 90-dB groups approached but did not reach a conventional level of significance, $F(1, 28) = 4.04$, $p < .10$. The 65-dB versus 70-dB comparison yielded an F ratio of less than 1.

By the last trial of noise preexposure the animals in the 65-, 70-, and 80-dB conditions were evidencing little suppression to the noise stimulus, the median suppression ratios for these respective conditions were .49, .51, and .53. The animals in the 90-dB condition, however, were still displaying considerable suppression to the noise, as the median suppression ratio for this group was .41. An analysis of variance on this data was significant, $F(3, 27) = 4.52$, $p < .025$. (The data from one animal in the 80-dB condition was lost because of an equipment failure.) It is quite clear that this F reflects the fact that the animals in the 90-dB group suppressed more to the noise than did those in the other conditions.

Light preexposure. There was no evidence that prior exposure to the noise stimulus influenced the subjects' unconditioned responding to the light CS. On the first light presentation, the median suppression ratios were .40, .41, .35, .34, and .40, respectively, for the 0-, 65-, 70-, 80-, and 90-dB conditions. An analysis of variance on these data yielded an F ratio which failed to approach statistical significance, $F(3, 35) = 1.21$, $p > .25$.

Conditioning and extinction. The median suppression ratios for each of the five groups during conditioning and extinction are presented in Figure 4. It is quite clear that the four groups of subjects preexposed to the noise prior to light presentations (i.e., the 65-, 70-, 80-, and 90-dB conditions) displayed more conditioned suppression than did those subjects only preexposed to the light, Group 0. The subjects in the latter condition showed very little evidence of conditioning over the first 4 trials of training, whereas, by Trial 4, each of the groups first preexposed to the noise was

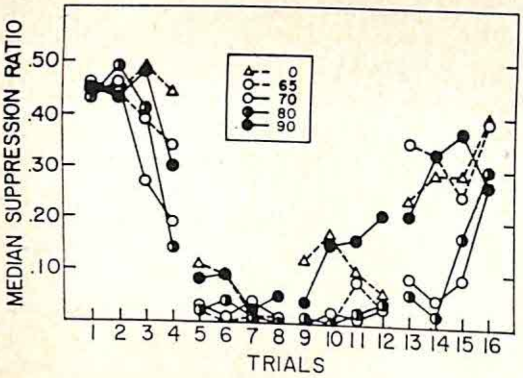

FIGURE 4. Median suppression ratios for each condition as a function of acquisition and extinction trials (Experiment 4).

displaying evidence of conditioning. In addition, it should be noted that there was a suggestion on Trial 4 that the 65- and 90-dB groups were somewhat less suppressed than the 70- and 80-dB conditions. On the last day of extinction (i.e., Trials 13–16) testing, in fact, the animals in the 65- and 90-dB groups were not distinguishable from the animals in the 0 condition, whereas the 70- and 80-dB conditions clearly were displaying more suppression than the 0 group. Thus, there appears to be some evidence of a nonmonotonic relationship between noise intensity during preexposure and amount of conditioning to a preexposed light stimulus.

Two separate analyses were performed on these data. The first analysis was on the acquisition data and used each subject's average suppression ratio over the first four trials of conditioning (Day 1). Orthogonal comparisons performed on these data revealed that the 0 condition differed reliably from the 65-, 70-, 80-, and 90-dB groups, $F(1, 35) = 4.61$, $p < .05$. Comparisons among the groups preexposed to the noise all yielded $F\text{s} < 1$. The second analysis was performed on the extinction data and used each subject's suppression ratio averaged across Trials 13–16 (the last day of extinction). An analysis of variance yielded a reliable group effect, $F(4, 35) = 2.70$, $p < .05$. Subsequent t tests revealed that the 0 condition was reliably different from the 70- and 80-dB groups, $t(35) = 2.46$, $p < .025$, and $t(35) = 2.06$, $p < .05$, respectively. There was no evidence, however, of any difference between the 0 group and the 65 and 90 conditions, $t\text{s} < 1$ for both comparisons. This pattern of statistical results, thus, is consistent with the suggestion in Figure 4 of a nonmonotonic relationship between the intensity of the preexposed noise and subsequent latent inhibition to the light.

The results of Experiment 4, thus, further establish that prior exposure to one stimulus can reduce the degree to which presentations of a second cue produce latent inhibition. In Experiments 2 and 3, it was found that preexposure to a light stimulus can attenuate the degree to which noise presentations result in a latently inhibited noise. In Experiment 4, it was found that preexposure to a noise stimulus can also alter the degree to which subsequent light presentations produce a latently inhibited light.

The results of this study also suggest that there may be a nonmonotonic relationship between the intensity of the noise during preexposure and the extent to which preexposure to the light retards conditioning. It is possible that the most intense noise had aversive properties (e.g., LoLordo, 1967; Rescorla, 1973) which in some unknown way interacted with the normal tendency of prior noise presentations to attenuate the effectiveness of light preexposure in producing latent inhibition.

GENERAL DISCUSSION

Previous research by Mackintosh (1973) has suggested that the number of separable stimulus events to which an organism is preexposed might be an important determiner of the latent inhibition effect. The several experiments reported here have clearly revealed that this is the case. First, the observation initially reported by Mackintosh (1973)—that preexposing the subjects to two stimuli simultaneously reduces the latent inhibition effect—was also found (cf. Experiment 2). More important, it was also discovered that prior exposure to one stimulus, S_1, can significantly attenuate the degree to which presentations of a second stimulus, S_2, retard conditioning to S_2. This is true if S_1 is a light and S_2 is a noise (Experiments 2 and 3) or if S_1 is a noise and S_2 is a light (Experiment 4). Thus, this line of research appears to have uncovered a new phenomenon which hereafter is referred to as the S_1–S_2 effect. As was previously noted, that such an effect is observed is somewhat surprising, as there were reasons to suspect that prior exposure to S_1, if anything, would have just the opposite effect on the development of latent inhibition to S_2. The counterintuitive S_1–S_2 effect thus presents an interesting theoretical problem.

Before discussing some alternative conceptions of the S_1–S_2 effect, it is necessary to have some theoretical assumptions about the mechanisms involved in latent inhibition per se. It seems reasonable to assume that the retarded rate of conditioning observed to a preexposed stimulus reflects in some way the fact that the subject has acquired some representation of its experience with that cue. A theory of latent inhibition then must be able to describe how such a representation can produce a retarded rate of conditioning. Wagner (in press) recently has developed an information-processing view of "habituation-like" phenomena, which offers a plausible account of how this might occur. His view elaborates upon previously developed models designed especially for interpreting a number of Pavlovian conditioning phenomena (e.g., Kamin, 1969; Rescorla & Wagner, 1972; Wagner, Rudy, & Whitlow, 1973) and has much in common with recent approaches to the human memory system (e.g., Atkinson & Shiffrin, 1968; Atkinson & Wickens, 1971). The model Wagner offers thus distinguishes between a limited-capacity, transient short-term memory (STM) and a relatively permanent, unlimited long-term memory (LTM). The critical proposition for present purposes is that so long as an event is represented in STM, its actual occurrence results in less processing than would otherwise occur. In the case applicable to latent inhibition, this would be interpreted to mean that a stimulus present in STM at the time of a conditioning episode is less likely to receive the additional processing necessary for it to become associated with the unconditioned stimulus. Wagner further assumes that there are two ways in which a stimulus event can be represented in STM. One way is via a self-generation process, by which a recent experience of a cue per se places its representation into STM. The second is via a retrieval process by which static contextual stimulus elements (S_C) of the experimental situation activate or, in Wagner's term, "prime" from LTM a representation of the previously experienced cue. Such priming is possible because Wagner assumes that repeated presentations of a cue result in the development of associations between it and S_C. A stimulus, thus, might be latently inhibited either (a) because its recent presentation has activated a self-generated representation of itself in STM or (b) because repeated presentations of the cue provided the opportunity for associations between S_C and the preexposed stimulus to be acquired, such that S_C can prime the cue's representation into STM.

Within this theoretical framework, the S_1–S_2 effect implies that S_2 is less primed at the time of conditioning than would be the case if S_1 presentations had not been given prior to S_2 preexposure. There appear to be two conceptually distinct reasons that S_2 may not be as well represented at the time of the conditioning episode when S_1 also has been preexposed. One conception which may be termed a *storage-failure* position, emphasizes the possiblity that prior S_1 presentations influenced the operations of the mechanism responsible for the acquisition or storage of S_2's representation; S_2 presentations were not processed in a way which allowed for storage of its representation in memory. Another conception may be termed a *retrieval-failure* view. This position allows that a representation of S_2 was acquired, but that prior presentations of S_1 interfered with the ability of S_C to retrieve a representation of S_2 at the time of the conditioning episode.

A retrieval failure approach to the problem, for example, might be developed more fully by interfacing Wagner's (in press) priming mechanism with the principles of the so-called associative-interference theory of forgetting (cf. Kintsch, 1970). It might reasonably be assumed that since both S_1 and S_2 were experienced in the same experimental context, independent associations were established between S_C and S_1 and S_2. Recall that Wagner's priming theory of latent inhibition requires that a cue's representation be primed into STM at the time of conditioning. On this reasoning, less latent inhibition occurs to S_2 because it is not as well primed into STM as otherwise would have been the case if S_1 had not also been preexposed. If one assumes that S_2 is less well represented because S_C also primes S_1's representation into STM, then one might expect the S_1–S_2 preexposure effect. In other words, the S_1–S_2 effect might be a proactive interference phenomenon, the preestablished S_C–S_1 association interfering or competing with the activation of S_2's representation in STM.

Using the basic theoretical framework provided by Wagner (in press) one might, with several additional assumptions, also develop a storage-failure account of the S_1–S_2 effect. For this type of theory, two classes of assumptions need to be made. One concerns how to conceptualize the stimulus events that are to be represented in LTM, the other concerns how to conceptualize the learning or storage process that gives rise to the organism's representation of its experience with these events.

Consider the following assumptions about the phasic stimulus events (i.e., the noise and light), which served as conditional stimuli in the present experiments. First, it is assumed that such events consist of a number of attributes (e.g., Bower, 1967; Spear, 1973). For example, the noise can be characterized by its intensity, frequency, duration, onset, and offset properties. Second, it is assumed that the attributes of a particular stimulus can be partitioned into two sets: those that are unique and serve to define it and those that are shared with other stimulus events occurring in the experimental situation. To represent this idea more clearly, two stimuli, S_1 and S_2, can be conceptualized as consisting of $s_a + s_x$ and $s_b + s_x$, respectively, where s_a and s_b are unique attribute sets and s_x a common set.

The next set of assumptions concerns the mechanisms that are responsible for inducing the formation of the critical associations between S_C and the various attributes (s_a, s_b, and s_x) of the preexposed stimuli, S_1 and S_2. The first assumption is that if an attribute's representation is not primed into STM, its presentation evokes a hypothetical arousal reaction. The important theoretical property of this reaction is that it modulates the consolidation, in LTM, of associations between S_C and the various attributes of the phasic stimulus. The stronger the arousal reaction the more likely it is that such associations are formed. This assumption is critical, thus it is worth noting that Konorski (1967, p. 87) made a similar assumption in his general theory of perceptual organization. The unique aspect of the proposition offered here is that the magnitude of the arousal reaction is determined by the number of attributes of a stimulus event that are not primed into STM by S_C.

It would be expected from this set of assumptions that one effect of prior S_1 presentations would be that the arousal reaction to S_2 would be attenuated relative to when S_1 has not been presented. This is because S_1 presentations would provide the opportunity for S_C-s_x associations to occur. Thus only the unique attribute set, s_b, of S_2 would be novel during presentations of S_2; the common attribute set, s_x, would be primed into STM and not contribute toward the magnitude of the arousal reaction. The consequence of this matter should be that the consolidation of associations between S_C and s_b should be less likely when S_2 exposure is preceded by presentations of S_1.

Consider now the state of affairs operative at the time of conditioning to S_2. For animals only exposed to S_2, both S_C-s_b and S_C-s_x associations presumably have been acquired, thus, S_C should prime both s_b and s_x into STM. For animals preexposed to S_1

and S_2, however, S_C should prime s_x, but s_b should be much less primed. If conditioning to S_2 is determined mostly by the extent to which s_b is primed, then one would expect the S_1–S_2 effect. Animals preexposed to S_1 and S_2 should condition more quickly to S_2 than animals only preexposed to S_2, because S_C should more strongly prime s_b for the latter animals.

It should be noted that in order to have the rate of conditioning determined by the difference in strength of the S_C-s_b associations, one must also assume that the common attribute set, s_x, contributes equivalently to conditioning to S_2 for the two conditions. One might assume that, although subjects preexposed to both S_1 and S_2 have had more presentations of s_x than those only presented with S_2, the number of S_2 presentations given the latter subjects was sufficient to have the S_C-s_x association at asymptotic strength. Alternatively, one might assume that s_x does not acquire conditioned properties as a consequence of S_2-US pairings. Either of these assumptions has the net effect of allowing conditioning to S_2 to be determined by the differences in strength of the S_C-s_b associations.

It should be appreciated that the storage-failure and retrieval-failure alternative approaches to the S_1–S_2 effect described above are intended to be heuristic devices for guiding future research of this problem. Both theories have in common the fact that they provide a means whereby a stimulus might be conditioned to as if it were novel, even though the subject previously had experienced it. The assumed mechanisms whereby this is accomplished, however, have different implications concerning the boundary conditions of the S_1–S_2 effect. For instance, the storage-failure position implies that the order in which the two cues are experienced determines which cue displays a reduced latent inhibition effect. From the storage-failure account, it should be the case that the degree of latent inhibition observed to S_1 is not interfered with by subsequent S_2 presentations. This should be the case because at the time of S_1 preexposures both s_b and s_x are novel, thus setting the conditions for S_C to become associated with both s_b and s_x. Subsequent S_2 presentations should, if anything, only strengthen the S_C-s_x associations and increase the observed latent inhibition effect with S_1. On the other hand, a retrieval-failure theory which attributes the S_1–S_2 effect to competition between S_C-S_1 and S_C-S_2 associations would allow for the possibility that subsequent S_2 presentations might also retroactively reduce the degree of latent inhibition to S_1.

Wagner (in press) also has made the assumption that so-called habituation phenomena reflect the development of associations between the stimulus context and a representation of the repeatedly presented stimulus. The ability of a stimulus to evoke an unconditioned reaction is seen as being diminished as the stimulus context becomes able to prime its representation into STM. Granted this assumption, both the storage-failure and retrieval-failure positions described above have implications for what should be observed during preconditioning presentations of S_2 alone. The storage-failure position would imply that the subjects' unconditioned reaction to S_2 should be weakened or reduced by prior presentations of S_1. This is because it is assumed that S_1 and S_2 share a common attribute set, s_x. In contrast, the retrieval-failure notion would imply that prior presentations of S_1 might interfere with the rate of habituation to S_2. This is because the preestablished S_C-S_1 association should result in S_C priming S_1's representation, which would in turn compete or interfere with the priming of S_2. Observations were made in Experiments 2, 3, and 4 which were potentially relevant to these assertions, as unconditioned suppression to S_2 was evaluated following presentations of S_1. There was no evidence from these studies, however, that unconditioned suppression evoked by S_2 was altered by prior presentations of S_1. It is difficult, however, to assess what such null effects mean for the theoretical positions under consideration. It may be the case that unconditioned suppression of the bar press response for food is simply not a sensitive index to the influence of S_1 presentations on the processing of S_2. Perhaps more direct indices such as changes in the subjects' autonomic response system might be more sensitive indices of this influence. Unfortunately, the existing literature on habituation of autonomic response systems (e.g., Graham, 1973) has very little to offer on this matter. Although there are a number of studies that have examined the same subject's unconditioned reaction to different stimulus events (e.g., Graham, 1973), there appears to be no systematic examination of the rate of habituation to S_2 as a function of prior S_1 presentations. It is clear that additional research employing more sensitive behavioral measures is needed for determining the influence of S_1 presentations on the unconditioned reaction to S_2.

Further consideration of the role of s_x, the common set of attributes of S_1 and S_2, in conditioning also may prove to be important. It was noted that one might assume, for the conditioning situation employed in the present studies, that s_x was not a part of the stimulus complex which acquired conditioned properties. This assumption is not as unreasonable as it might appear at first glance. Suppose, for example, a major component of s_x was the sudden onset characteristic of both S_1 and S_2. Since the CS-US interval in the studies reported here was 3 min., it might well be the case that the onset attribute does not become associated with the US. If this were the case, then in the CER situation where relatively long CS-US intervals are used one would see no generalized latent inhibition, if following only S_1 preexposure, subjects were conditioned to S_2. If, however, the CS-US interval was relatively brief (e.g., 1–3 sec), such that the onset attributes might be part of the stimulus complex that acquires conditioned properties, prior S_1 presentations might produce a generalized latent inhibition effect to S_2.

Such reasoning also suggests that there may be some kinds of conditioning situations in which the reverse of the S_1–S_2 effect found here would be obtained. If the common attribute set plays a dominant role in the evocation of a conditioned response, prior exposure to S_1 might enhance the extent to which S_2 preexposures would produce latent inhibition of S_2. The rabbit eyelid conditioning situation, in which very brief (e.g., 250–1,000 msec) CS-US intervals are essential to good conditioning, may be one such setting. Whether this reasoning is valid of course can only be determined by additional research.

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Long-Term Retention of the Habituation of Lick Suppression and Startle Response Produced by a Single Auditory Stimulus

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Response decrement produced by a single, 2-sec, 98 dB, auditory stimulus in rats was retained without detectable loss for intervals ranging from approximately 30 sec to 30 days. Response decrements produced by stimulation over these intervals reached an apparent asymptote after 2 or 3 stimulus presentations. These response decrements were unambiguously associated with stimulus presentation for the lick suppression measure, but nonspecific decremental effects may have contributed to the startle response decrements. After these long-term decrements had reached asymptote, 300 stimulus presentations with 1-sec interstimulus intervals produced further response decrements, but these decrements recovered completely within 24 hr., responsiveness returning to the previously established long-term asymptote. The data show that unreinforced stimulus presentations can produce both relatively permanent response decrements and apparently independent short-term decrements. The data also suggest that long-term retention may be more generally characteristic of habituation than is usually assumed.

Leaton (1974) has shown significant retention, with little or no recovery for up to 42 days, of the habituation of lick suppression to auditory stimuli in rats. This long-term retention was obtained following habituation-training sessions of 10 trials per day (with an average intertrial interval of approximately 18 sec) for 4 consecutive days. These data are consistent with several recent reports that have shown relatively long-term habituation following repetitive stimulation. Retention of habituation of heart rate and galvanic skin response has been shown in human subjects over intervals of 24 hr. to 1 wk. (Bishop & Kimmel, 1969; Harding & Rundle, 1969; Keen, Chase, & Graham, 1965; Kimmel & Goldstein, 1967). Retention of habituation for at least 6 days has been found for the acoustic startle response in rats (Davis, 1972; Moyer, 1963), for up to 8 days for reflexes in the spinal kitten (Kozak, MacFarlane, & Westerman, 1962), for up to 21 days for a withdrawal reflex in *Aplysia* (Carew, Pinsker, & Kandel, 1972), for up to 4 days for withdrawal in the earthworm (Gardner, 1968), and for at least 30 days for the electroencephalographic-arousal response in rats (Leaton & Buck, 1971).

These data show that habituation, at least under some stimulus–response conditions, must be considered a relatively permanent change in behavior. Such data are consistent with Thorpe's (1956) definition of habituation as a "relatively permanent waning of a response as a result of repeated stimulation" (p. 54). However, there are numerous demonstrations of habituated responses that show apparently complete recovery within a few minutes or hours (e.g., Askew, Leibrecht, & Ratner, 1969; Pinsker, Kupfermann, Castellucci, & Kandel, 1970; Prosser & Hunter, 1936; Thompson & Spencer, 1966), and Thompson and Spencer (1966) consider spontaneous recovery one of the major defining characteristics of habituation. The extent of the retention of habituation

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can be expected to depend on many characteristics of the experimental situation, but there has been little success in specifying those characteristics. It seems to be generally assumed, at least implicitly, that long-term retention of habituation occurs only after prolonged stimulation. Hinde (1970), for example, suggests that some habituation of complex responses may be permanent, while long-term habituation of simpler responses occurs only after long continued stimulation.

Little attention has been given to the extent of the retention of habituation produced by a single stimulus presentation. Does a single stimulus exposure produce only a transient effect which must be reinforced by repeated stimulation to produce long-term effects, or can a single stimulus exposure produce a relatively permanent response decrement? Konorski (1948) reported habituation of the orientation reflex with interstimulus intervals of 24 hr. or longer, but he provides no experimental details. Leaton (1974), measuring the habituation of lick suppression in rats, found that the habituation produced by a single, 2-sec auditory stimulus was retained for at least 30 days. The present experiment focused on the retention of habituation of both lick suppression and startle response produced by a single auditory stimulus. Following the initial retention tests, repeated trials were given with the retention interval as the interstimulus interval. It was assumed that if there were any differences associated with the retention interval, they would accumulate and be accentuated over trials.

EXPERIMENT 1

Method

Subjects. Fifty experimentally naive male albino rats (Holtzman), 120–150-days-old at the time of pretraining, were individually housed and allowed ad-lib access to food. They were maintained throughout the experiment on a 23-hr., 40-min. water deprivation schedule which began approximately 4 wk. before any training. Animals were allowed free access to water in their home cages for 15 min. immediately following each experimental session.

Apparatus. Animals were tested in an apparatus basically similar in design to that described by Hoffman and Fleshler (1964). The test chamber was constructed of perforated Plexiglas with a grid floor. The inside dimensions were $20 \times 12 \times 14$ cm. The chamber was mounted between compression springs attached to a rigid superstructure. A bar magnet was attached to the bottom of the chamber, and the magnet rode inside a rigidly mounted electrical coil. Movements of the chamber moved the magnet within the coil and induced a current in the coil which was amplified and recorded on paper with a Grass Model-7 Polygraph. In addition, the amplified startle record was full-wave rectified and input to an integrator (Grass Model 7P10) which traced a ramp function, the amplitude of which was directly proportional to the "total accumulated area" under the startle tracing. The integrator was adjusted to reset to baseline every second, and the difference between the integrator amplitude, in mm, for the 1-sec period before and the 1-sec period after stimulus onset was taken as the measure of startle. Holding circuits allowed the presentation of a stimulus only at the instant of integrator reset. Movements of the chamber were highly damped, and even vigorous movements of the subject produced movements of the chamber too small to be visible.

To allow for the simultaneous measurement of lick suppression, the rat had access to a drinking tube through a 2×1-cm slot 5.5 cm above the grid floor and centered in the short wall of the chamber. The drinking tube was suspended, free of the startle chamber, just outside the chamber wall. Licking was recorded on one channel of the polygraph and on electromechanical counters by means of a drinkometer circuit. The duration of lick suppression was measured from the time the rat stopped licking during stimulus presentation until continuous licking resumed for 1 sec.

A 20-cm speaker (Allied Electronics) was centered 13.5 cm from the long wall of the box. Continuous white noise (72 dB, re $20\mu N/m^2$) helped mask extraneous auditory stimuli. The test stimuli were 10-kHz, 98-dB tones of 2-sec duration superimposed on the background of white noise. Intensity of the tone and white noise were measured with a General Radio Sound-Level Meter, Type 1551-C (20-kHz setting), with the microphone placed approximately where a drinking rat's head would be. The entire apparatus was housed inside an unlighted, sound-attenuating chamber.

Procedure. Briefly, retention of habituation of lick suppression and startle response was tested in independent groups several seconds, 1 day, 7 days, 15 days, and 30 days after the application of a single auditory stimulus. The 15-day group received its first stimulus on the day the 30-day group received its second stimulus in order to evaluate the possible contribution of nonspecific effects (e.g., sickness, weight loss, weight gain) to response decrements associated with the very long interval between stimuli. Testing continued for all groups for a total of six trials in order to evaluate possible differences in the course of habituation. The interstimulus interval for the continued testing for each group was the same as the initial retention interval for that group.

Prior to any habituation training, all rats were given 18 daily 5-min. sessions in the test chamber to establish asymptotic licking rates. By the end of this pretraining, all animals licked almost continuously throughout a session. Licking, as has been reported before (Shaeffer & Premack, 1961), occurred in an all-or-none fashion. The animal was either licking at a rate of five to six licks per second or was not licking. Because the animals maintained a relatively constant posture while licking, the baseline activity in the startle record was stable throughout a session and between sessions. The integrated amplitude of baseline activity in the startle record for 1-sec epochs ranged typically between 2 and 5 mm.

The first test session was given on the day following the last pretraining day. Each animal of every 5 animals run was assigned to a different experimental group. One of each block of 5 animals was arbitrarily assigned to a group that received no stimulus presentations during the first test session. This group is described further, below. The other 4 animals in each block of 5 received one, 2-sec auditory stimulus (10-kHz, 98 dB) and were assigned to groups based on the amplitude of the startle response and the duration of lick suppression evoked by the first stimulus. Group assignments were made immediately following the first tone, so that those animals that were to receive additional tones during the first session could be identified before the second tone was due. In this way five groups of 10 animals each were created. Four of the groups received one or more tone presentations during the first session and were approximately matched on the basis of their first trial responsiveness. The first stimulus for this and all subsequent test sessions was presented following the first 40 sec of the session. In addition, the animal was required to have made 200 total licks and have licked continuously for the 10 sec prior to stimulus presentation.

One of the four matched groups (Group Sec) was given five additional tone presentations during this first session. Each of these additional stimuli was presented 10 sec after the resumption of licking following the previous stimulus, provided that the 10-sec continuous licking requirement had been met. The mean interval between the first and second tone for this group was 28.3 sec (median = 23.0). The mean interstimulus interval for the six trials was 18.2 sec (median = 15.8). None of the other groups received additional tone presentations during the first test session. Each of the other three matched groups received five additional test sessions with one tone presentation during each session. There were 24 hr. (1 day) between sessions (and thus between tones) for Group 1, 7 days between sessions for Group 7, and 30 days between sessions for Group 30.

The fifth group of 10 rats received no tone presentations during the first test session, although in all other respects it was treated identically to the other groups. The animals in this group (Group NS-15) received their first tone presentation on the same day that the Group 30 rats received their second tone presentation. Following their first tone presentation the rats in Group NS-15 received five additional test sessions with 15 days between each session and one tone presentation during each session.

Groups 7, 30, and NS-15 were given pretraining sessions on the 5 days immediately preceding each of the test sessions following the first test session. These pretraining sessions served to reinstate asymptotic licking. All pretraining and test sessions were 5 min. long.

Results

The results for the startle response measure for all five groups are shown in Figure 1. (Two rats in Group 30 died before the completion of six trials. Their inclusion for trials that they did complete in no way altered the statistical conclusions, but they were dropped from all data treatment.)

Since the decrease in responsiveness from Trial 1 to Trial 2 directly reflects the habituation associated with a single stimulus presentation, the data were first analyzed over these two trials for Groups Sec, 1, 7, and 30. A repeated measures analysis of variance for these two trials for the four groups showed that the decrease from Trial 1 to Trial 2 was significant, $F(1, 34) = 23.78$, $p < .001$. Neither the group difference nor the interaction approached significance, both Fs < 1. Analysis of the data for these four groups over six trials showed that the decrease over trials was significant, $F(5, 170) = 14.90$, $p < .001$. Again, neither the group difference, $F < 1$, nor the interaction, $F(15, 170) = 1.40$, was significant.

In order to evaluate more clearly the retention of habituation following a single stimulus presentation, apart from any nonspecific factors which could lead to response decrements over very long intervals, Trial 1 for Group NS-15 was compared to Trial 2 for Group 30. These trials for these two groups came on the same day. The rats were the same age and had received the same experimental treatment except that the rats in Group 30 had received one tone presenta-

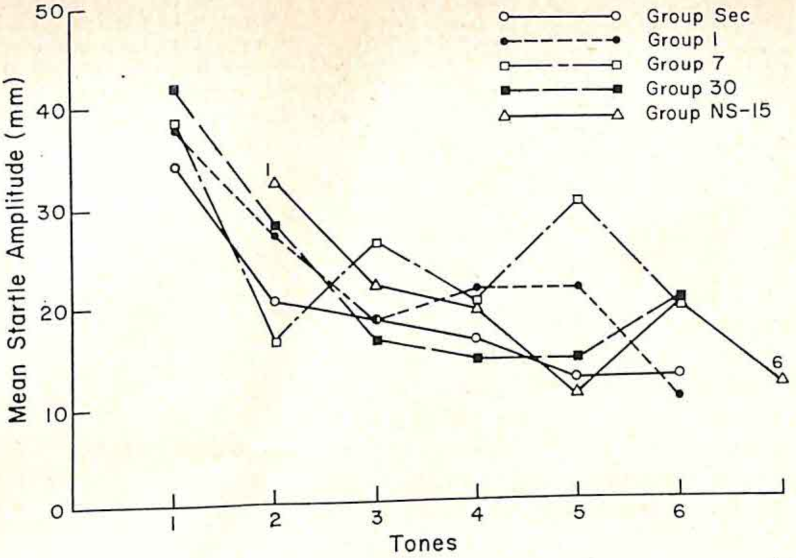

FIGURE 1. Mean startle response amplitude for the five groups in Experiment 1. (The curve for Group NS-15 is displaced to the right to indicate that the first tone for that group did not come on the same day as for the other groups. The 1 and 6 on the curve for Group NS-15 show that group's first and sixth tone.)

tion 30 days earlier. This comparison was not significant for the startle response measure, $t < 1$. In addition, analysis of variance comparing Trials 2–6 for Group 30 to Trials 1–5 for Group NS-15 showed only a significant decrease across trials, $F(4, 64) = 4.96$, $p < .01$. Neither the group difference nor the interaction approached significance, both $F\text{s} < 1$. It should also be noted that the decrease from Trial 1 to Trial 2 for Group 30 was not significant, $t(7) = 1.63$, although the decrease over six trials for this group was, $F(5, 35) = 4.59$, $p < .01$.

The results for the lick suppression measure are shown in Figure 2. For statistical analysis duration of lick suppression was transformed to log $(x + 1)$ in order to stabilize the variances. Repeated measures analysis of variance for Groups Sec, 1, 7, and 30 showed that the decrease from Trial 1 to Trial 2 was significant, $F(1, 34) = 46.30$, $p < .001$. The group difference was not significant, $F(3, 34) = 1.80$, but the interaction was, $F(3, 34) = 3.36$, $p < .05$. In an effort to locate the source of this interaction, the data were reanalyzed excluding Group Sec. Again the trial effect was highly significant, $F(1, 25) = 53.80$, but now neither the group difference nor the interaction approached significance, both $F\text{s} < 1$. Analysis of the data for these four groups over six trials showed that the decrease over trials was significant, $F(5, 170) = 34.94$, $p < .001$. There was also a significant group difference, $F(3, 34) = 2.89$, $p < .05$, but there was no significant interaction, $F(15, 170) = 1.15$. Reanalysis of these data excluding Group Sec again yielded a highly significant trials effect, $F(5, 125) = 42.77$. The interaction was still not significant, $F(10, 125) = 1.02$, but the group difference now did not approach significance, $F < 1$.

Comparing Trial 1 for Group NS-15 to Trial 2 for Group 30, trials that came on the same day for these two groups, revealed a significantly shorter duration of lick suppression for Group 30, $t(16) = 3.05$, $p < .01$. Analysis of variance comparing Trials 2–6 for Group 30 to Trials 1–5 for Group NS-15 showed a significant decrease across trials, $F(4, 64) = 11.83$, $p < .001$, and a significant interaction, $F(4, 64) = 5.14$, $p < .01$, but the group difference was of only borderline significance, $F(1, 16) = 3.53$, $p = .07$.

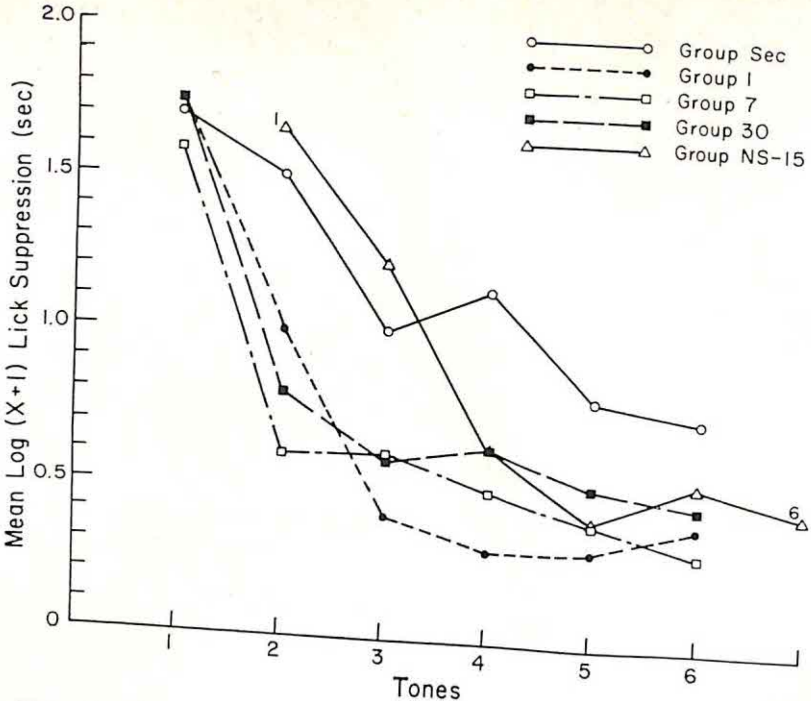

FIGURE 2. Mean duration of lick suppression for the five groups in Experiment 1. (The display for Group NS-15 is the same as in Figure 1.)

Correlations were calculated between the startle response measure and the lick suppression measure both for response amplitude and for rate of habituation. The overall pattern of correlations did not suggest significant correlation between measures. Overall correlations of response amplitude were calculated for each group. These correlations ranged in value from .27 to .54, and none were significant. Of the 30 correlation coefficients resulting from correlating the two measures for each trial for each group, 11 were negative, 19 were positive, and 3 of the positive correlations were significant. There was no pattern in these 3 significant correlations. To correlate rate of habituation, the data over six trials for each subject for both measures was fit to a negative exponential equation, $\log y = \log K + A \log N$, by a least squares analysis. In this way an exponent describing the rate of response decrement was derived for each subject for each measure. The correlations between these exponents were then calculated. The overall correlation for all groups was .20. The correlations for individual groups ranged from −.18 to .67. The only significant correlation was for Group 7, $r(8) = .67$, $p < .05$.

Discussion

The results of Experiment 1 showed that the habituation produced by the presentation of a single, 2-sec auditory stimulus can be retained for at least 30 days. This conclusion is unambiguous for the lick suppression measure. Except for the lick suppression data for Group Sec, there was no evidence that the response decrement from the first to the second tone presentation depended on the interval between stimuli. Continued testing for six trials over these same intervals revealed no differences that could have resulted from differential cumulative effects. That the response decrements were related primarily to stimulus presentations and not to any nonspecific factors (e.g., aging, poor health, weight gains, weight losses) was indicated by the results for Group NS-15. This group began its test sequence on the day Group 30 received Trial 2 and by this time Group 1 had completed testing and

Group 7 had completed Trial 4. Group NS-15's duration of lick suppression on Trial 1 was significantly longer than the Trial 2 lick suppression for Group 30 and was comparable to the Trial 1 duration of lick suppression for all of the other groups.

The only evidence in this experiment that the retention of habituation of lick suppression was related to the interval between stimuli was the lick suppression data for the group with the shortest test intervals (Group Sec). The difference associated with this group was in the direction of showing "poorer retention" over shorter intervals. This difference may be related to a sensitization effect (Groves & Thompson, 1970) that decays over longer intervals. Indeed, Davis (1972) showed a "deepening" of habituation following a 24-hr. rest interval, which he related to the differential decay of sensitization and habituation. However, Group Sec did not differ from the other groups on the startle response measure, and it is not clear why sensitization would affect lick suppression without also affecting the startle response.

The startle response measure, like the lick suppression measure, showed significant decrements from Trial 1 to Trial 2 and over six trials; the overall analysis revealed no significant differences associated with the interval between stimuli. However, the comparison between Trial 1 for Group NS-15 and Trial 2 for Group 30 was not significant, making it impossible to relate the startle response decrements unambiguously to stimulus presentations. Indeed, the response decrement from Trial 1 to Trial 2 for Group 30, when this group was analyzed alone, was not significant, although this group did show significant response decrements over the six trials.

There are several possible factors which could account for the difference between the startle response measure and the lick suppression measure on this dimension. The basic retention function for the two measures could be different, certainly a reasonable possibility given the failure to find significant correlations between the two measures either in response amplitude or in rate of response decrement. The startle response measure could be more sensitive to nonspecific effects or less sensitive to minimal amounts of habituation. Or Group NS-15, which was not matched with the other groups, could simply have had a lower baseline startle because of sampling error. These factors (and possibly others) operating alone or in combination, could account for the differences noted.

Overall these data strongly suggest that, at least under some experimental conditions, the response decrement produced by a single, brief auditory stimulus is permanent. Although permanence has not been demonstrated, and is theoretically impossible to demonstrate, one would expect a transient process to show some recovery over the range of intervals tested here. In the case of lick suppression the response decrements were clearly related to stimulus presentation, and barring the possibility of permanent sensory damage, can be ascribed to habituation. Sensory damage seems a very unlikely possibility given the brevity and intensity of the stimulation. The nature of the response decrements in startle response remains unclear, but startle response did show a long-term component strongly suggestive of habituation.

EXPERIMENT 2

The habituation demonstrated in Experiment 1 showed no signs of spontaneous recovery over the intervals tested. This is in contrast to many reports in the literature which have shown spontaneous recovery of habituated responses over a few seconds or minutes. Although retention of habituation may be expected to depend upon a number of variables, these extreme discrepancies in recovery time suggest that a meaningful distinction can be drawn between short-term and long-term habituation. Certainly, many investigators have proposed such a distinction, and some have suggested that the two processes are mediated by independent underlying mechanisms (i.e., Groves & Lynch, 1972; Hinde, 1970; Horn, 1970; Sharpless & Jasper, 1956; Thompson

& Spencer, 1966; Wagner, in press; Whitlow, 1975).

These considerations, along with the data from Experiment 1, suggest the possibility that a single stimulus presentation initiates at least two decremental processes. One is a relatively permanent response decrement like that shown in Experiment 1. The other is a decremental process with a relatively short time course. Davis (1970) made explicit assumptions about the operation of such short-term and long-term processes in interpreting his data, and Wagner (in press) reviewed a series of studies that strongly suggest the action of two such processes. Wagner's theoretical position, derived from such data, assumes that responsiveness on a given trial in a habituation sequence is depressed by a short-term process generated by the immediately preceding trial and a separable long-term process reflecting the overall history of prior stimulation. If, following a stimulus presentation, additional stimulation falls within the time course of the short-term process, further response decrements will occur, but these decrements will recover rapidly over periods of nonstimulation. However, recovery from these short-term decrements will only reach the decremental asymptote of the long-term process.

Thus, it is expected that stimulation with long interstimulus intervals (longer than approximately 20 sec under the conditions of Experiment 1) will produce a relatively permanent response decrement, as in Experiment 1, which reaches an apparent asymptote after only a few stimulus presentations. It is assumed that under these conditions any short-term decremental process initiated by a stimulus decays before presentation of the next stimulus. If massed stimulus presentations are given after this long-term process has reached asymptote, then further response decrements will occur, but these response decrements will recover rapidly over periods of nonstimulation and recovery will be to the asymptote of the long-term process. Experiment 2 was designed to test this possibility.

Method

Subjects. Twelve experimentally naive male albino rats (Holtzman) were 120-days-old at the time of initial testing. They were individually housed and had free access to both food and water throughout the experiment.

Apparatus. The apparatus was the same as that used in Experiment 1 except that the drinking tube was removed. Only startle response was measured.

Procedure. All rats were given 5-min. pretest sessions in the test chamber on 5 consecutive days. No stimuli were presented during these sessions. On each of the next 10 days each rat received one presentation of a 2-sec, 98-dB, 10-kHz tone during each 5-min. test session. The tone was presented between the 2nd and 4th min. of the session. On the day following the last of these 10 test sessions, each rat was given 300 presentations of the auditory stimulus during 1 20-min. test session, with a 1-sec interval between the offset of one stimulus and the onset of the next. The first tone in the sequence of 300 was presented at the same time into the session as was the single stimulus during the previous 10 test sessions. The remaining 299 tones were programmed automatically every second. Each rat was given 3 additional 5-min. test sessions on the 3 days following the 300-trial session. One stimulus was given during each of these 3 sessions. The stimulus was the same 2-sec, 98-dB, 10-kHz tone for all trials during all sessions.

Results

Figure 3 shows the mean amplitude of the startle response for all phases of the experiment. The left panel shows the 11 trials with a 24-hr interstimulus interval. The first trial of the 300-trial session is shown as Trial 11 because it was procedurally the same as the other trials in this phase. This trial is also included in the first block of 30 trials shown in the center panel. The center panel shows the startle amplitude over 10 blocks of 30 trials during the 300-trial session. The right panel shows the startle amplitude for the 3 daily trials that followed the 300-trial session.

Repeated measures analysis of variance showed that the decrease in responsiveness over the first 11 trials with a 24-hr. interstimulus interval was significant, $F(10, 110) = 3.61$, $p < .001$. Startle amplitude decreased further during the 300-trial session with the 1-sec interstimulus interval. Analysis of variance showed that the decrease over the 10 blocks of 30 trials was signifi-

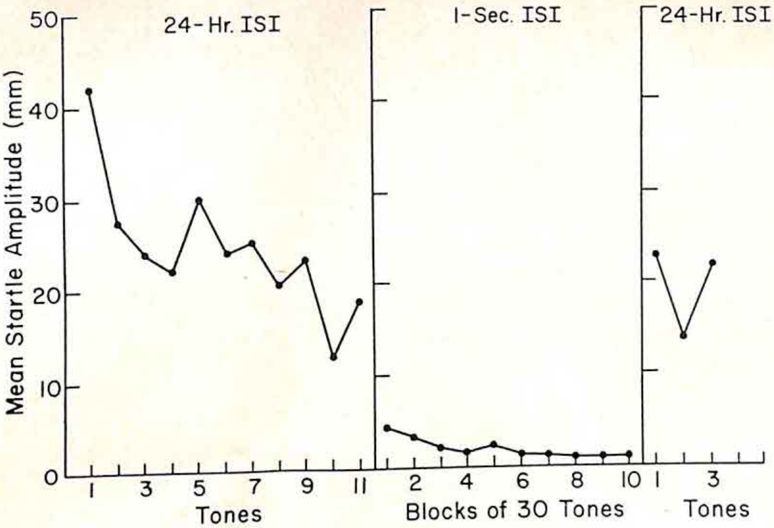

FIGURE 3. Mean startle response amplitude for the three phases of Experiment 2. (The left panel shows trial-by-trial responsiveness with a 24-hr. interstimulus interval. The center panel shows responsiveness for the 300-trial session with a 1-sec interstimulus interval plotted over blocks of 30 trials. The right panel shows responsiveness for the 3 trials that followed the 300-trial session.)

cant, $F(9, 99) = 5.37$, $p < .001$. The decrease in startle amplitude with the 1-sec interstimulus interval occurred rapidly, most of the reduction coming within the first 10 trials. Indeed, there was an immediate reduction in startle amplitude to the first stimulus that followed the previous stimulus by 1 sec, although this decrease was of only borderline significance, $t(11) = 2.03$, $p = .06$. The data for the first 10 trials are shown in Figure 4. Analysis showed that the decrease over these 10 trials was significant, $F(9, 99) = 4.19$, $p < .001$. There was further reduction in responsiveness associated with the 1-sec interstimulus interval as shown by the significance of the decrease from the 2nd to the 10th block of 30 trials, $F(8, 88) = 4.27$, $p < .001$.

As shown in the right panel of Figure 3, spontaneous recovery from the habituation produced by the 300 massed trials occurred within 24 hr. Startle response for the last block of 30 trials was significantly lower than that for the first trial following the 300-trial session, $t(11) = 3.86$, $p < .01$, and for the mean of the 3 trials following the 300-trial session, $t(11) = 7.11$, $p < .001$. Recovery was complete in that the first trial following the 300-trial session was not significantly different from the first trial of the 300-trial session, $t < 1$, nor was the mean of the 3 trials following the 300-trial session significantly different from the mean of the 3 trials that preceded the 300-trial session, $t < 1$. However, the first trial after the 300-trial session and the mean of the 3 trials were significantly lower than Trial 1 in the initial 24-hr. interstimulus interval sequence, $t(11) = 2.29$, $p < .05$ and $t(11) = 3.62$, $p < .01$, respectively.

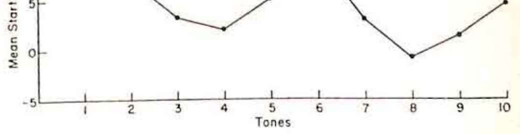

FIGURE 4. Mean startle response amplitude for the first 10 trials of the 300-trial session in Experiment 2.

General Discussion

The results found with the 24-hr. interstimulus interval in Experiment 2 basically replicated Experiment 1. A significant reduction in startle response amplitude across trials was shown. It is difficult to establish with any degree of certainty where the response reached asymptote, but as in Experiment 1 the primary response decrement occurred between Trials 1 and 2. The basic similarity between the 24-hr. habituation curve and the comparable curve in Experiment 1 suggests that there was no fundamental difference in startle response habituation depending on whether the animal was water deprived and drinking at the time of stimulus presentation, as in Experiment 1, or was nondeprived, as in Experiment 2. However, these data cannot be seen as inconsistent with reports that deprivation suppresses the amplitude of the startle response (Fechter & Ison, 1972; Ison & Krauter, 1975).

The results of the 300-trial session showed that the startle response amplitude could be reduced further when stimulus presentations were sufficiently massed. This outcome is consistent with many reports that habituation is inversely related to the length of the interstimulus interval (cf. Thompson & Spencer, 1966) and the immediate reduction in responsiveness with the 1-sec interstimulus interval basically replicates Davis's (1970) finding comparing a 2-sec and 16-sec interstimulus interval. The response decrements resulting from repetitive stimulation at a 1-sec interstimulus interval recovered, apparently completely, within 24 hr., and recovery was to the asymptote of habituation that was established with a 24-hr. interstimulus interval. That these data show better retention of habituation following training with a 24-hr. interstimulus interval than following training with a 1-sec interstimulus interval is consistent with Davis's (1970) report of better retention following training with a 16-sec as compared to a 2-sec interstimulus interval.

Whether the response decrements produced by stimulation at 1-sec intervals was short-term habituation in the usual sense of the term or whether it was related to fatigue, receptor adaptation, "prepulse inhibition" (Ison & Hammond, 1971), or a response "refractory period" (Wilson & Groves, 1973) cannot be unambiguously concluded from these data. That significant response decrements continued beyond the first block of 30 trials may indicate a habituation component, but some recent data (Ison, Zuckerman, & Russo, 1975) suggest that refractory effects may summate over trials. Some form of fatigue or sensory adaptation could well have been operating, and there can be no doubt that response decrements associated with stimulation over brief intervals involve the interaction of several processes. Davis (1970), for example, suggests a distinction between short-term and long-term habituation based on differences on the order of 1 min. Whatever processes are involved it should be clear that the response decrements and spontaneous recovery seen here following stimulation at 1-sec intervals were basically similar to effects typically called habituation in the literature.

The results of these two experiments have clearly shown the operation of at least two decremental processes resulting from nonreinforced stimulus presentations. One was a relatively permanent effect which showed no evidence of recovery for up to 30 days without stimulation. The second was a relatively short-term process which was revealed when stimulation was sufficiently massed (i.e., interstimulus intervals somewhere shorter than approximately 20 sec under the conditions of these experiments, although the time course of the short-term process would be expected to vary with experimental conditions), and which showed complete spontaneous recovery within 24 hr.

These results can easily be incorporated within the framework of Wagner's (in press) theory. A stimulus presentation initiates a short-term decremental process, and the extent to which the short-term process is allowed to run its course without additional stimulation determines the degree to which it is transferred into a long-term decremental process. If the interstimulus interval is longer than the time course of the short-term process, the maximum amount of transfer to the long-term process would occur and response decrements would be independent of the interstimulus interval. Such was the case in Experiment 1 with interstimulus intervals ranging from 20 sec to 30 days. In addition, the results of that experiment showed that, at least for some response systems, a single stimulus can produce a significant long-term process, and relatively few stimuli can drive the long-term process to its decremental asymptote. If additional stimulation falls within the time course of the short-term process produced by a previous stimulus, the resulting response decrement will be inversely related to the interstimulus interval, and spontaneous recovery will occur over an interval equal to the duration of the short-term process. The recovery will be to the decremental level reached by the long-term process. Thus in Experiment 2, stimulation with a 24-hr. interstimulus interval produced an apparently asymptotic long-term decrement. Additional stimulation with a 1-sec interstimulus interval produced further response decrements because the interstimulus interval was shorter than the time course of the short-term process. These decrements, however, recovered completely within 24 hr., and recovery was to the asymptote of the long-term process.

The two processes revealed in these experiments demand different underlying neural mechanisms. The mechanism that stored the long-term habituation cannot be identical to the mechanism that produced the short-term decrements since the insertion of 300 massed trials after the long-term process had reached an apparent asymptote did not affect responsiveness 24 hr. later. Under these conditions the two processes were independent, but they could interact at some level of habituation training. If, for example, habituation training had begun with the massed-trial presentations, it is possible that the long-term process might have been disrupted.

It is also possible that, if stimulation had been continued for 300 trials with the test intervals used in Experiment 1, further long-term response decrements would have occurred. If this were so it would suggest an interesting interaction between number of trials and the interstimulus interval. Wagner's theory apparently predicts such an interaction because with sufficiently massed trials little would be transferred from the short-term to the long-term process. Under the conditions of Experiment 2, 300 stimulus presentations at a 1-sec interstimulus interval did not produce any long-term effect beyond that already produced by relatively few stimuli at a 24-hr. interstimulus interval.

There is no reason to assume that all response systems with all stimulus parameters with all species would show the long-term, relatively permanent, habituation shown in these experiments. However, habituation within at least one response system (i.e., the acoustic startle response in rats) that has been typically characterized by rapid spontaneous recovery (but see Davis, 1972; Moyer, 1963) has been shown here to have a significant long-term component. Long-term retention may be more generally characteristic of habituation than is usually assumed. Most of the experimental paradigms used to study habituation are not designed to detect long-term effects, particularly the effects produced by a single stimulus event. Hundreds of trials are often given over relatively short time periods, and data are often presented in blocks of 10 or more trials. Following such a paradigm the long-term decrements shown here, which reached asymptote after only 2 or 3 stimulus presentations, could easily be overlooked. If, for example, the data from the first 310 trials in Experiment 2 (see Figure 3) had been plotted and analyzed in blocks of 10 or more trials, ignoring the intertrial interval differences, an orderly habituation curve would have been produced, and recovery would have been complete 24 hr. following Trial 310. In addition, if a percentage response measure with, say, a 2–5-mm response criterion had been taken, rather than an amplitude measure, no retention would have been seen with the 24-hr. interstimulus interval, since all animals responded on almost all trials. Habituation would have been seen with the 1-sec interstimulus interval, and recovery would have been complete within 24 hr. It is also possible, as suggested earlier, that if a habituation series began with massed stimulus presentations, the long-term process could be disrupted. For these, and possibly other, reasons relatively short-term response decrements may be the only reported effects even within response systems that have robust long-term habituation components.

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ERRATUM

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Interaction Versus Independence of Startle-Modification Processes in the Rat

Christopher L. Stitt, Howard S. Hoffman, and Roger R. Marsh
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In a series of experiments it was found that the amount of inhibition produced by a weak acoustic stimulus occurring 64 msec prior to a startle-eliciting stimulus is unaffected by a second weak acoustic stimulus occurring 4 msec prior to the startle-eliciting stimulus. Likewise, the amount of latency reduction produced by an antecedent stimulus with a 4-msec lead time is unaffected by the presence of an antecedent stimulus with a 64-msec lead time. Finally, it was found that both the amount of inhibition produced by a prestimulus with a 64-msec lead time and the amount of latency reduction produced by a prestimulus with a 4-msec lead time are independent of the intensity of the startle-eliciting stimulus. These findings suggest that the inhibition effect is independent of the latency reduction effect and that both are independent of the absolute level of activation in the neural mechanisms responsible for the overt startle response.

When exposed to a sudden intense acoustic stimulus, the rat's initial overt reaction often consists of an abrupt crouchlike gesture. Previous investigations of this response, the acoustic startle reflex, have indicated that events in the sensory environment preceding the startle stimulus affect both the amplitude and latency of the reaction. Hoffman and Searle (1965) found that a single clicklike auditory stimulus inhibited the rat's response to a startle-eliciting auditory stimulus that followed the first sound by 20–2,000 msec. Subsequently, this inhibitory effect has been elicited by the offset of an otherwise present acoustic stimulus (Stitt, Hoffman, & Marsh, 1973), by shifts in the frequency spectrum of an ongoing noise (Stitt, Hoffman, Marsh, & Boskoff, 1974), and by a brief light flash (Ison & Hammond, 1971; Schwartz, Hoffman, Stitt, & Marsh, 1976). These studies also show that if these changes in the sensory environment precede the startle stimulus by only 4–5 msec, inhibition is not observed; rather, the latency of the response is reduced.

Inhibition and latency reduction occur whether the change in the sensory environment and the startle-eliciting stimulus are from the same or different modalities (Ison & Hammond, 1971; Schwartz et al., 1976). They do not depend on the presence of any detectable reaction to the change in the sensory environment (Hammond, McAdam, & Ison, 1972), nor do they require associative pairing of the sensory change and the startle stimulus (Ison, Hammond, & Krauter, 1973). The amount of inhibition or latency reduction is a positive function of the amount of change in the sensory environment (Hoffman & Wible, 1970; Schwartz et al., 1976), and both effects follow nonmonotonic time courses, reaching a maximum at a temporal interval of 40–120 msec for inhibition and 2–4 msec for latency reduction (Stitt et al., 1973). Although identical in the above respects, the two effects have different absolute thresholds. Inhibition is produced by smaller changes in the sensory environment than is latency reduction (Marsh, Hoffman, Stitt, & Schwartz, 1975).

Based upon these findings we have suggested that the two modification effects are mediated by separate though perhaps inter-

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related neural processes (Marsh et al., 1975). And more recently we have proposed (a) that inhibition of the reflex results from the action of a central inhibitory mechanism engaged by the change in the sensory environment and (b) that latency reduction reflects partial activation of the neural pathways mediating the startle reflex (Stitt, Hoffmann, Marsh, & Schwartz, in press). The present series of experiments was designed to investigate how the inhibition and latency-reduction effects interact with each other and, furthermore, how they change with changes in the intensity of the startle-eliciting stimulus.

EXPERIMENT 1

This study examined the effects of an inhibition-producing prepulse (p_1) and a latency-reducing prepulse (p_2) presented together or singly before a startle-eliciting stimulus (S). The times of presentation of p_1 and p_2 were chosen to approximate that of maximal inhibition (for p_1) and latency reduction (for p_2).

The interest in this experiment is how the combinations of prepulses might alter the amplitude-inhibition and latency-reduction effects. The appearance of certain sorts of interactions might elucidate basic aspects of the workings of the neural processes mediating startle. One particularly meaningful question is whether p_1 and p_2 have independent access to the startle reflex or whether, in contrast, the effect of p_1 would dominate and block the effect of p_2.

There were three possible outcomes of the experiment. First, as noted above, the effect of p_1 could dominate and block the effect of p_2. Second, the two prepulses could, in some manner, summate to produce increased inhibition and/or latency reduction. Finally, the two prepulses could produce entirely independent effects.

Method

Subjects. The subjects were five experimentally naive male albino rats obtained from West Jersey Biological Supply Company. They were approximately 120 days old at the time of testing.

Apparatus. The device for the assessment of startle consisted of a 20.3 × 12.7 × 15.2-cm chamber supported by a relatively stiff suspension system such that vibrations of the chamber were transmitted to a magnet located in a stationary coil. When the subject made a sudden movement (as during startle), an electrical current was generated in the coil. Since the current was proportional to the chamber's rate of motion, the device was sensitive to the rather rapid startle response while remaining comparatively insensitive to the slower movements associated with the rat's general activity. The output of the coil was filtered (to remove high-frequency artifacts resulting from the impact of intense acoustic signals), amplified, rectified, and passed to a simple circuit that integrated the output by permitting it to charge a capacitor for 100 msec after the startle stimulus onset. The voltage of the capacitor was then displayed on a high-impedance digital voltmeter with storage capability.

For the measurement of response latencies, the output of the coil was also displayed on one channel of a Tektronix storage oscilloscope (Type 654). The signal appears as a decaying complex wave with an abrupt onset and a fundamental frequency of 70 Hz, which reflects the resonant frequency of the test cage and suspension. Latency was measured to the onset of this signal from a mark synchronized with the arrival of the startle stimulus in the test cage. The observer who made the latency measurements was unaware of the stimulus configuration to avoid bias in these judgments.

Startle stimuli were generated by feeding the output of a General Radio random-noise generator (Type 1390-B) through a Krohn-Hite (Model 310-AB) band-pass filter (set for 1,000–10,000 Hz to match the frequency characteristics of the speaker) to a Grason-Stadler electronic switch (Model 829-C), operated by a Grason-Stadler interval timer (Model 471-1). This arrangement permitted control over the durations of the signals as well as their rise-decay time. The signal was then fed from the electronic switch through a Hewlett-Packard 350-AR decade attenuator, a Crown DC 300 audio power amplifier, and a semiconductor gate to a speaker consisting of an Altec 230C midrange driver with a 60-cm exponential horn. The semiconductor gate was a General Electric X12 Triac, which appears as an open circuit until triggered and then permits current flow in both directions. The triggering circuit was such that the acoustic stimulus would fire the Triac, but the unwanted internal noise of the equipment would not.

Acoustic prepulse stimuli were generated by a similar system and fed to a separate Altec 230C midrange driver. The intensities of both the prepulse and startle stimuli were assessed with a Hewlett-Packard ½-in. (12.7-mm) calibrated condenser microphone which was placed inside the subject's chamber. The output of the microphone was fed through a General Radio precision sound-level meter (Model 1561-A) and then displayed

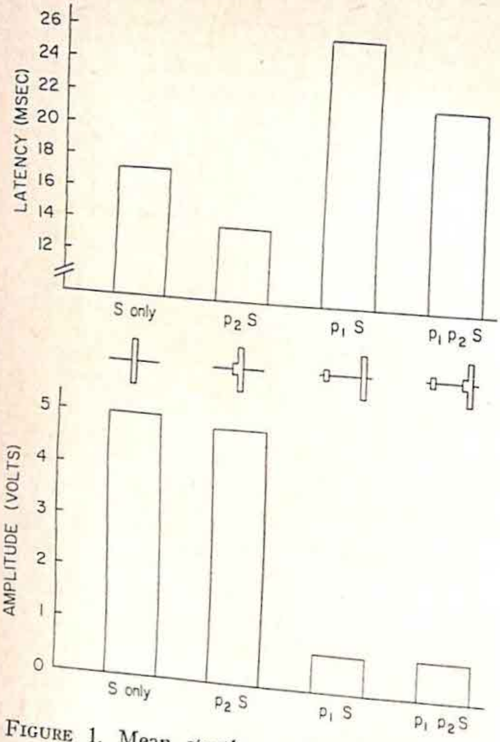

FIGURE 1. Mean startle amplitude and latency (across subjects) as a function of startle stimulus configuration. (The stimulus configurations are represented schematically between the graphs.)

on one channel of a Tektronix dual-channel storage oscilloscope (Type 654). The procedure was to take a large number of measurements, moving the microphone after each measurement until the entire space of the subject's chamber had been sampled, and then to define the intensity of the signal as the mean of the distribution of measurements.

The research was conducted in an Industrial Acoustics Corporation ventilated, double-wall isolation room. This facility has an ambient noise level less than 20 dB for frequencies above 100 Hz. (All intensities given here are sound pressure level re 20 $\mu N/m^2$.) A closed-circuit television system was used to provide visual observation of the subjects.

Procedure. In this experiment, four stimulus configurations were employed. The first consisted of a broadband random-noise startle stimulus (intensity, 120 dB; duration, 20 msec; rise-decay time, .1 msec). The second stimulus configuration consisted of the 120-dB startle stimulus presented 64 msec after the onset of a weak broadband random-noise signal (intensity, 70 dB; duration 20 msec; rise-decay time, .1 msec). The third stimulus configuration was identical to the second, except the interval between the onset of the weak random-noise signal and the startle-eliciting stimulus was reduced to 4 msec. The final stimulus configuration consisted of the 120-dB startle stimulus preceded by two weak random-noise pulses, which occurred 64 msec and 4 msec prior to the startle-eliciting stimulus.

Each subject received eight presentations of each of the four different stimulus configurations in each of two daily sessions. The order of presentation was determined by a random numbers table for each session. Stimulus presentations occurred at 30-sec intervals, and each subject was allowed 5 min. to adapt to the chamber before each session.

Results and Discussion

Figure 1 shows the mean latency and amplitude, averaged across subjects and sessions, of startle responses elicited by the four stimulus configurations. Separate Treatment × Treatment × Subjects analyses of variance (Bruning & Kintz, 1968) performed on the amplitude and latency data revealed the following results.

First, given a lead time of 64 msec (p_1), a relatively weak prepulse (which does not itself evoke detectable startle) inhibits the startle response to the intense signal, $F(1, 4) = 13.27$, $p < .05$. These small, inhibited responses tend to have longer latencies than the large, uninhibited responses, $F(1, 4) = 359$, $p < .01$. This combination of results is in agreement with prior investigations of the startle reflex (Hoffman & Searle, 1968; Schwartz et al., 1976; Stitt et al., in press).

Second, given a lead time of 4 msec (p_2), a relatively weak prepulse reduces the latency of the startle response to the intense signal, $F(1, 4) = 185$, $p < .01$, but does not affect the amplitude of that response, $F(1, 4) = 1.66$, *ns*. Once again these findings replicate previous reports (Hoffman & Searle, 1968; Hoffman & Wible, 1970; Schwartz et al., 1976; Stitt et al., 1973, 1974, in press).

Finally, and most importantly, the presence of a prepulse with a 64-msec lead time (p_1) does not alter the effect of a prepulse with a 4-msec lead time (p_2), $F(1, 4) = 2.5$, *ns*, and, likewise, the presence of p_2 does not alter the effect of p_1, $F(1, 4) = 1.29$, *ns*. In other words, p_2 tended to reduce the latency of a small, inhibited response by the same amount that it reduced the latency of a large, uninhibited response. Likewise,

p_1 inhibited reactions with reduced latency (i.e., reactions to stimuli containing p_2) as much as it inhibited the longer latency reactions to stimuli that contained no p_2.

This lack of interaction indicates that the two prepulses are producing entirely independent effects. Such a finding provides evidence that p_1 and p_2 have independent access to the neural processes responsible for the organization of startle.

EXPERIMENT 2

This experiment examined a second aspect of the startle-response modification phenomenon. In particular, the work sought to specify the relation between amount of modification (inhibition in the first case and latency reduction in the second case) and startle-response amplitude and latency as determined by the intensity of the eliciting stimulus. For each type of startle-response modification there were two possible outcomes of this experiment. First, inhibition and/or latency reduction could be subtractive—modifying the startle response by a constant amount, regardless of its amplitude and latency as determined by the intensity of the eliciting stimulus. Second, inhibition and/or latency reduction could be multiplicative—modifying the startle response by a constant proportion of its amplitude or latency as determined by the intensity of the eliciting stimulus. Therefore, the items of greatest interest in the analyses of this study were the interactions between the startle-stimulus intensity and prepulse factors. For either type of modification, a large interaction term would contradict subtractive modification and support multiplicative modification.

Method

Subjects. Ten experimentally naive male albino rats obtained from West Jersey Biological Supply Company were used in this study. They were approximately 150 days old at the time of testing.

Apparatus and procedure. The apparatus used in this experiment was identical to that used in Experiment 1. Two basic stimulus patterns were presented to all subjects. The first consisted of a broadband random-noise startle stimulus (duration 20 msec; rise-decay time, .1 msec) that had an intensity of either 90, 100, 110, or 120 dB. The second pattern consisted of the same-intensity startle stimuli presented at a period in time after the onset of a weak broadband random-noise prepulse (intensity, 70 dB; duration, 20 msec; rise-decay time, .1 msec). For five of the animals (those in the inhibition group), the temporal interval between onset of the prepulse and onset of the startle stimulus was 64 msec. For the other five subjects (the animals in the latency-reduction group), this interval was 4 msec.

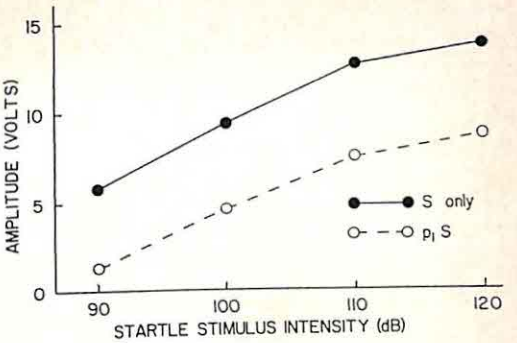

Figure 2. Mean startle amplitude (across subjects) as a function of startle stimulus intensity. (The solid curve indicates responses to those stimulus configurations consisting of only a startle stimulus. The dashed curve indicates responses to those stimulus configurations in which a weak signal preceded the startle stimulus by 64 msec.)

Each subject received six presentations of each of the eight different stimulus configurations in each of two daily sessions. The order of presentation was determined by a random numbers table for each session. Stimulus presentations occurred at 30-sec intervals, and each subject was allowed 5 min. to adapt to the chamber before each session.

Results and Discussion

Figure 2 shows the mean amplitude, averaged across subjects and sessions, of startle responses produced by the inhibition group animals. A Treatment × Treatment × Subjects analysis of variance (Bruning & Kintz, 1968) was used to evaluate the reliability of the observed trends. This test revealed a significant effect of startle-stimulus intensity, $F(3, 12) = 104.66$, $p < .01$. In agreement with earlier work of Hoffman and Searle (1968), the present data indicate that startle response amplitude increases with increased startle-stimulus intensity. The analysis also revealed, as anticipated from all the previously cited work, a significant inhibitory effect of the prepulse, $F(1, 4) = 116.28$, $p < .01$.

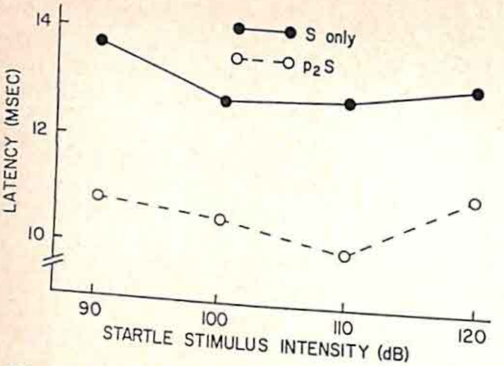

FIGURE 3. Mean startle latency (across subjects) as a function of startle stimulus intensity. (The solid curve indicates responses to those stimulus configurations consisting of only a startle stimulus. The dashed curve indicates responses to those stimulus configurations in which a weak signal preceded the startle stimulus by 64 msec.)

Finally, and most importantly, the data indicated no interaction between the startle-stimulus intensity and the prepulse factor, $F(3, 12) = .19$, *ns*. As noted in the introduction to this study, the absence of a significant interaction term suggests that inhibition is a subtractive effect—a prepulse with a 64-msec lead time reduces a startle response by an amount that is independent of the intensity of the eliciting stimulus.

Figure 3 shows the mean latency, averaged across subjects and sessions, of startle responses produced by the latency-reduction group animals. A Treatment × Treatment × Subjects analysis of variance indicated a significant effect of both main factors: $F(3, 12) = 6.75$, $p < .01$, for startle-stimulus intensity; $F(1, 4) = 75.01$, $p < .01$, for prepulse. The relatively small effect of startle stimulus intensity is in agreement with the work of Hoffman and Searle (1968), and the effect of the prepulse is in agreement with all of the previously cited work.

The analysis of the present data also indicated that there was not a significant interaction of the startle stimulus intensity and prepulse factors, $F(3, 12) = .19$, *ns*. As noted above, the lack of an interaction suggests that the effect of the prepulse is subtractive—a prepulse with a 4-msec lead time reduces the latency of the response by a constant amount, regardless of the latency of the response as determined by the intensity of the eliciting stimulus. The conclusion to be drawn from this study is clear: The amount of startle-response modification (either inhibition or latency reduction) observed in a given stimulus configuration is determined by the parameters of the prepulse and is unaffected by the intensity of the startle-eliciting stimulus.

CONCLUSIONS

These findings are consistent with those of the earlier investigations which first suggested that the inhibition and latency-reduction effects are mediated by separate neural processes (Marsh et al., 1975; Stitt et al., in press). They extend the original reports, however, in two directions. First, the present data reveal that the two processes have essentially independent access to the neural mechanism responsible for the organization of the startle reflex. Second, the present work suggests that both modification effects are independent of the level of activity of the basic reflex mechanism as determined by startle-eliciting stimulus intensity.

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Modification of Ducklings' Filial Behavior by Aversive Stimulation

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Three experiments examined the effects of aversive stimulation in the context of imprinting. In Experiment 1, newly hatched ducklings were electrically shocked in either the presence or absence of an imprinting stimulus, and both their tendency to follow the stimulus when it was presented alone and their preference for it over novel stimuli were subsequently assessed. The results indicated that the shock enhanced the subjects' tendency to follow the imprinting stimulus in the nonchoice situation regardless of whether the ducklings were shocked in its presence or absence. If, however, the electrical shocks were associated with the presence of the imprinting stimulus, they also reversed the ducklings' usual preference for the imprinting object over the novel stimuli. Comparable effects were obtained in the second experiment in which 5-day-old ducklings were electrically shocked in either the presence or absence of a novel, fear-eliciting imprinting stimulus. In Experiment 3, electrical shock was administered to newly hatched ducklings independently of their exposure to an imprinting stimulus, and it was found that, relative to older ducklings that received the same treatment, the newly hatched subjects subsequently displayed less emotionality in fear-eliciting situations. The results were interpreted as suggesting that there are at least three distinct effects of aversive stimulation in the context of imprinting—motivational, associative, and developmental.

Prior studies focused on the behavioral effects of aversive stimulation in the context of imprinting have led to discrepant conclusions regarding the nature of the processes that underlie the formation of social attachments by precocial birds. The present series of experiments was designed to extend the previous analyses of these effects in an attempt to reconcile the discrepant conclusions they have thus far led to and thereby to clarify our understanding of the basic nature of the imprinting phenomenon itself.

In more detail, early investigations (Kovach & Hess, 1963; Moltz, Rosenblum, & Halikas, 1959) revealed that if aversive stimulation is administered to a newly hatched precocial bird while it is being exposed to an imprinting stimulus, the subject follows the moving object more closely than it otherwise would. On the basis of this finding (herein referred to as the *enhanced following effect*), Hess (1959) has suggested that aversive stimulation causes an "enhancement of the effectiveness of the imprinting experience" (p. 59), and both he (1959, 1964) and Eibl-Eibesfeldt (1970) have concluded that the imprinting phenomenon, rather than being based on either a discriminative or associative learning process, instead reflects a unique form of learning. If imprinting were based on a traditional learning process, they argue, then the administration of aversive stimulation would produce a decrement rather than an incre-

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ment in the subject's responsiveness to an imprinting stimulus.

Recent work by Barrett (1972), however, has expanded Hess's analysis of the effects of aversive stimulation and demonstrated that under appropriate circumstances such stimulation does indeed produce a decrement in a bird's responsiveness to an imprinting stimulus. More specifically, Barrett electrically shocked newly hatched ducklings in the presence of a repeatedly presented and withdrawn moving stimulus and then measured the subjects' preference for that stimulus by giving them the choice to approach either it or a novel but similar object. In contrast to the enhanced responsiveness that is evident when the imprinting stimulus is presented alone, Barrett now found that the ducklings chose to approach the novel object rather than the one to which they had been imprinted. On the basis of this *reduced preference effect,* Barrett, unlike Hess and Eibl-Eibesfeldt, argued that "aversive stimuli have effects . . . in the context of imprinting that are analogous to those [evident] under other circumstances typically characterized as learning" (p. 321).

Indeed, it seems possible that the reduced preference observed by Barrett may be explicable in terms of the traditional learning processes that Hoffman and Ratner (1973a) have recently suggested as constituting the underlying mechanism of the imprinting phenomenon. In contrast to Hess and Eibl-Eibesfeldt's assertion that imprinting represents a unique learning process, our reinforcement model of imprinting views the formation of a social attachment by a newly hatched precocial bird as being based on classical conditioning processes. More specifically, we proposed that through association with a motivationally significant form of stimulation (the movement of the imprinting stimulus), the initially neutral static features that characterize the particular stimulus to which a bird is being imprinted become familiar. Once familiarity with a particular stimulus is achieved, the age-related development of a tendency to react fearfully to novel moving objects is presumed to restrict the bird's emission of filial behavior to only the familiar stimulus, that is, the moving object to which it has been imprinted. In terms of this model, the reduced preference effect obtained by Barrett was due to the addition of another motivationally significant source of stimulation—the electrical shock—to the usual elements of the classical conditioning process that is postulated to be the basis of imprinting. Since this additional stimulation is aversive, its association with the presence of a repeatedly presented and withdrawn imprinting stimulus would be expected to have produced an attachment that was confounded by conditioned aversiveness and hence possibly characterized by a reduced preference for the imprinting object.

Taken together, both Hess's enhanced following and Barrett's reduced preference effects point to the conclusion that aversive stimulation has more than just one significant consequence in the context of imprinting. These consequences, when considered independently, have led to discrepant conclusions concerning the relationship between imprinting and more usual instances of learning. The purpose of the first experiment in the present series was to clarify this issue by closely examining the nature of the enhanced following and reduced preference effects within the same experimental context and then determining whether the findings are consistent with the implications of a traditional learning interpretation of the imprinting phenomenon.

EXPERIMENT 1

Experiment 1 was designed, in part, to clarify the enhanced following and reduced preference effects of aversive stimulation by delineating and possibly distinguishing between the necessary and sufficient conditions for their occurrence. In order to do so, different groups of newly hatched ducklings were electrically shocked in either the presence or absence of a repeatedly presented and withdrawn imprinting stimulus. Periodically, the subjects' tendency to follow the stimulus and their behavior when given the choice to approach either it or a novel object were assessed. This procedure, besides providing the opportunity to examine both following and preference behavior within the same experimental context, also entailed a significant extension of the procedures used in the previously cited studies by Hess and Barrett. More specificially, it allowed the assessment of the relative effects of associating aversive stimulation with the *absence* as well as the presence of an imprinting stimulus. Under these conditions, if either the enhanced following or reduced preference effect was observed in the subjects shocked in the presence of the imprinting stimulus but not those shocked in its absence, then the importance of a direct association between the imprinting stimulus and the aversive stimulation for the occurrence of that effect would be clearly demonstrated. On the other hand, if either effect was found to characterize the performance of the ducklings shocked in the absence of the stimulus as well as those shocked in its presence, then it would be clear that that effect was not subserved by such an association.

In addition to administering choice tests between the imprinting stimulus and a novel but *similar* stimulus (as done by Barrett), the design of Experiment 1 also entailed the use of a novel *dissimilar* stimulus to assess the degree of the ducklings' preference for the imprinting object. The novel stimulus employed by Barrett was sufficiently similar to the one to which the subjects had been imprinted to elicit filial behaviors when it was presented alone, regardless of whether the subjects had been shocked during their prior exposure to the imprinting stimulus. In contrast, the novel dissimilar object used here was not a member of the class of stimuli that included the imprinting stimulus, and its use allowed the assessment of the generality of Barrett's findings. More specifically, it revealed whether the administration of aversive stimulation creates a preference for a novel stimulus independently of the degree of similarity between it and the original imprinting object.

Method

Subjects

Twelve Khaki campbell ducklings (*Anas platyrhynchos domesticus*), hatched in a forced-air incubator from embryonated eggs obtained from George F. Shaw, Inc., West Chester, Pennsylvania, were used as subjects. As soon as an egg exhibited pip marks it was covered with a small plastic container (10 × 10 × 10 cm) which remained over the egg until 10–12 hr. after the duckling hatched. The use of this container minimized the visual stimulation the subjects received while in the incubator.

At 10–12 hr. posthatch, both the container and the duckling were removed from the incubator and the subject was placed in its permanent individual housing unit. This unit, located in a humidified room maintained at approximately 30 °C, consisted of a covered 15-gal. (56.6-l) container made of white translucent plastic and lined with a clear, heavy-duty polyethylene bag. The bag was partially filled with an absorbent bedding (San-i-cel), and food and water were continually available. The ducklings were kept in these units at all times except when they were transferred to the experimental apparatus for testing.

Apparatus

The experimental apparatus is illustrated in Figure 1. Basically, it consisted of a plywood box (184 × 76 × 76 cm) divided by a fine-mesh, stainless-steel screen into two compartments, a stimulus compartment (184 × 30 × 76 cm) and a subject compartment (184 × 46 × 76 cm), the floor of which was covered by a thin layer of San-i-cel. Lighting in the subject compartment was provided by two continuously illuminated 75 W incandescent bulbs mounted above the screen. These bulbs were positioned such that the light reflected from the screen prevented the subject from viewing into the stimulus compartment unless that compartment was also illuminated. Finally, the floor area of the subject compartment was divided into a 2 × 2 matrix with four equal-size quadrants (92 × 23 cm) by photocell units, permitting the continual assessment of the approximate location of the ducklings during the experimental procedures.

The *imprinting stimulus* consisted of a white rectangular foam rubber object (30 × 10 × 10 cm) mounted over a model train engine. In order to present the stimulus to a duckling in the subject compartment, two overhead 75 W incandescent bulbs in the stimulus compartment were illuminated, and the stimulus was moved back and forth along HO-gauge track at approximately 30 cm/sec. The track ran for the full length of the stimulus compartment, and switches that reversed the direction of the stimulus were located at each end of the track. In addition, a reversing switch was also located at the middle of the length of track for the experimental procedures that required that the path of the stimulus be confined to one-half the length of the stimulus compartment. Withdrawal of the imprinting stimulus was accomplished by stopping its movement and extinguishing the overhead lamps in the stimulus compartment. Arranged in this fashion, the experimenter could rapidly present and withdraw the imprinting stimulus as needed and control whether its path covered the full length of the stimulus compartment or just one-half of it.

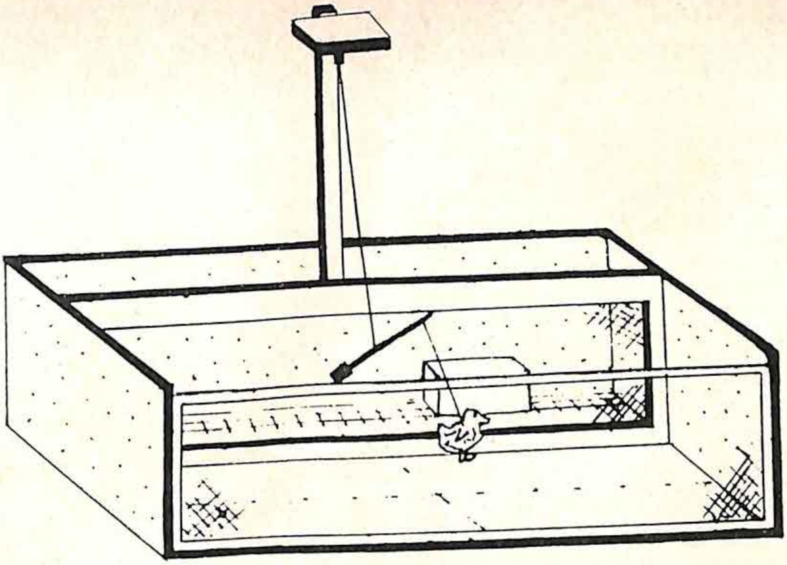

FIGURE 1. Apparatus used for imprinting and testing. (The broken lines on the floor of the subject compartment illustrate the four quadrants that were delineated by the photocell units.)

The novel similar stimulus was identical to the imprinting stimulus with the following modification: Black adhesive tape was used to form a cross-hatched pattern on the white foam rubber covering. As with the imprinting stimulus, the path of the novel similar stimulus could extend either the full length of the stimulus compartment or be restricted to one-half of it.

The novel dissimilar stimulus consisted of a rotating amber-colored lamp, similar to those used on the top of many police and ambulance vehicles (although of less intensity). Presentations and withdrawals of this stimulus were accomplished by either illuminating or extinguishing its bulb and simultaneously initiating or terminating its rotation. When the experimental procedure entailed the simultaneous presentation of this stimulus and the imprinting stimulus, the path of the imprinting stimulus was confined to one-half the length of the stimulus compartment, the novel dissimilar stimulus was placed in the center of the other half, and the overhead bulb in the half containing the novel dissimilar stimulus was disconnected.

Electric shock was delivered to the ducklings through permanently fastened wingbands (see Hoffman & Ratner, 1974, for full details), mounted on each subject when the bird was removed from the incubator. Basically, the wingbands consisted of 8-cm lengths of finely linked 14-carat gold chain that were permanently fastened to each end of a single 4-cm length of thin nylon thread. The thread was laid across the duckling's back, and each of the chains was wrapped twice around the base of a wing and attached to itself with a small piece of plastic adhesive. This arrangement permitted rapid mounting and adjustment of the wingbands and insured that they would neither slip off nor interfere with the bird's movements.

At the start of each experimental session, the wingbands were connected to two small, lightweight, miniature clamps. The wire from these clamps were sufficiently long to permit the duckling to move over the full area of the subject compartment and were connected to a multiple-circuit mercury commutator which allowed the whole shock delivery system to rotate with the duckling. In addition, a simple counterweight mechanism (made of a balsa-wood pole and appropriate weights) was used both to reduce the weight of the clamps on the subject's back and to take up any slack in the connecting wires (see Figure 1). Finally, the source of the electrical shock was an Applegate Constant-Current Stimulator.

All stimulus events were programmed through electromechanical equipment. An Esterline-Angus Events Recorder was used to record these events and the location of the duckling with respect to the four quadrants delineated in the subject compartment. In addition, the subjects were continually observed during the experimental procedures over a closed-circuit television system.

Procedure

Phase 1: Exposure to the imprinting stimulus. Upon being transferred from the incubator to its individual housing unit, each duckling was fitted with a pair of wingbands (as previously described) and randomly assigned to one of three equal-size groups ($n = 4$). At both 14- and 18-hr. posthatch, the subjects in each group were individually placed in the center of the experimental apparatus for a

20-min. session in which the imprinting stimulus was continually present and moving the full length of the stimulus compartment. Shocks were not administered during these sessions.

On both Days 2 and 3 posthatch, each duckling was again given two 20-min. sessions of exposure to the imprinting stimulus; unlike the continuous exposure on Day 1, however, the imprinting stimulus was now repeatedly present and absent for alternating 50-sec intervals. In addition, differential treatment of the subjects in each group was begun during these sessions. For the ducklings in one of the groups, an electric shock (1.0 mA intensity, 300 msec duration) was delivered 9 sec into each period of presentation of the imprinting stimulus and every 9 sec thereafter for the duration of each presentation. For the subjects in the second group, shocks were delivered at the same rate and intensity but during the periods of stimulus absence. The ducklings in the last group were not shocked in either the presence or absence of the imprinting stimulus, although they were connected to the shock delivery system so as to equate all aspects of the experimental procedure except the actual delivery of shock.

Immediately after each of the four sessions on Days 2 and 3 posthatch, the extent to which the subjects in each of the groups followed the imprinting stimulus was assessed. In order to do so, the ducklings were given a 5-min. test during which the imprinting stimulus continually moved back and forth the full length of the stimulus compartment; shocks were not administered to any of the subjects during these tests. Whenever the stimulus reversed direction and the duckling was in the quadrant closest to the activated reversals switch, a positive following measure was scored. Negative following measures, similarly, were scored whenever the duckling was in any of the other three quadrants at the instant of stimulus reversal. Using this technique, a duckling that followed the stimulus closely and consistently would yield all positive following measures; a subject that stayed by the path of the stimulus, on the other hand, but that did not actively follow the object would yield an equal number of positive and negative following measures. Active avoidance of the stimulus, finally, would be characterized by all of the following measures being negative.

Phase 2: Choice tests and exposure to the novel dissimilar stimulus. On Day 4 posthatch, the ducklings in each of the three groups were individually given two choice tests, one between the imprinting stimulus and the novel similar stimulus and one between the imprinting stimulus and the novel dissimilar stimulus. These tests were administered in a counterbalanced order—two of the four ducklings in each group had the former choice test first and the latter second; the other subjects in each group had the latter first and the former second. The interval between the choice tests ranged from 1 to 2 hr.

Each choice test lasted 15 min., and during this time the imprinting stimulus and novel stimulus were simultaneously presented and withdrawn 10 times in the different halves of the stimulus compartment. Each presentation of the two stimuli lasted 60 sec, and the periods of absence were 20 sec. long. During each of the stimuli presentations, the location of the duckling (with regard to whether it was in the quadrant closest to the imprinting stimulus or the quadrant closest to the novel stimulus) was continually monitored by the photocell units and recorded on the Esterline-Angus recorder. To compensate for any positional preferences that might have developed during the prior imprinting exposure, the location of the two stimuli was reversed after the 5th period of stimuli presentation.

On the fifth day posthatch, each duckling was individually exposed to the novel dissimilar stimulus. This exposure lasted 20 min.; the stimulus, continually visible and rotating, was located in the center of one half of the stimulus compartment. During the session, the position of the subjects relative to the location of the stimulus was continually monitored by the photocell units.

On the next day, each of the ducklings was again given a choice test between the imprinting stimulus and the novel dissimilar stimulus. The procedure of this test was identical to that of the two choice tests given on the fourth day posthatch. While shocks were not administered during these tests (or during the exposure to the novel dissimilar stimulus on Day 5), each subject was nonetheless connected to the shock-delivery system at these times.

Results

Following Measures

Following scores, representing the percentage of following measures from each of the four tests on Days 2 and 3 posthatch that were scored as positive, were calculated for each subject. Figure 2 illustrates the mean values of these scores for the ducklings in each group as a function of the repeated tests. As can be seen in this figure, the subjects that were not electrically shocked during the preceding imprinting sessions showed little tendency to follow the imprinting stimulus, although their performance indicates that they approached and stayed in the general vicinity of its path. Observation of these ducklings over the closed-circuit television system revealed that upon being placed in the experimental apparatus, these ducklings immediately approached the screen dividing the stimulus and subject compartments and

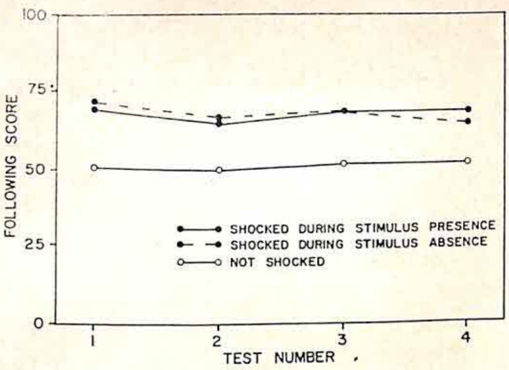

FIGURE 2. Following of the imprinting stimulus during the repeated 5-min. tests. (A score of 50 indicates that the subjects did not actively follow the stimulus but did stay in the vicinity of its path. Scores greater than 50 reflect increasingly greater amounts of active following. Shocks were not administered during these tests.)

usually sat dawn, quietly observing the stimulus.

In contrast to the behavior of the nonshocked subjects, the ducklings previously shocked in either the presence or absence of the imprinting stimulus were observed to follow actively the stimulus during the 5-min. test periods. As illustrated in Figure 2, the elevated following scores of these subjects reflect this performance, and a repeated-measures analysis of variance (Winer, 1962) revealed that the difference between these groups and the group of subjects not previously shocked was statistically reliable, $F(2, 9) = 7.9$, $p < .05$. Neither the test effect nor the Group × Test interaction was significant.

Choice Tests

Figure 3 illustrates the mean percentages of time that the ducklings in each of the three groups spent in the quadrants closest to the imprinting stimulus and novel stimuli when the stimuli were repeatedly presented during the three choice tests. The left vertical panel of this figure shows these measures for each of the groups on the choice test between the imprinting stimulus and the novel similar stimulus. The uppermost graph in this panel illustrates that the subjects previously shocked in the presence of the imprinting stimulus showed a strong preference for the novel similar stimulus when given the choice to approach either; during the periods of stimuli presentation, these ducklings spent approximately 70% of the time in proximity to the novel similar stimulus and only 20% in proximity to the im-

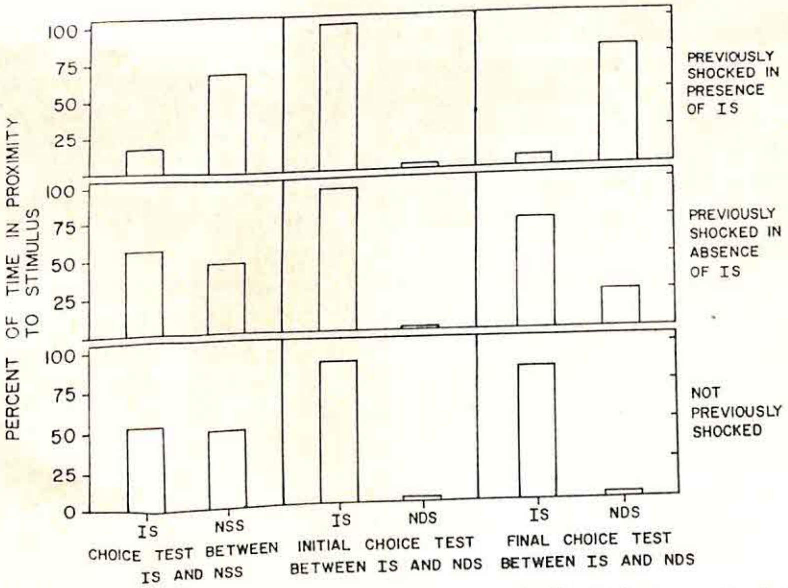

FIGURE 3. Approach to the imprinting stimulus and novel stimuli during each of the several choice tests. (IS, NSS, and NDS refer to the imprinting stimulus, novel similar stimulus, and novel dissimilar stimulus, respectively. Shocks were not administered during these tests.)

printing stimulus (the remaining 10% of time was spent in one of the two quadrants not in proximity to either of the two stimuli). The lower two figures in the left panel reveal that the ducklings previously shocked in the absence of the imprinting stimulus and those not previously shocked did not show a preference for either the imprinting or novel similar stimulus; approximately equal amounts of time were spent in proximity to the two stimuli. A two-factor, mixed-design analysis of variance (Winer, 1962) indicated that the trends depicted in the left panel of Figure 3 are statistically reliable; while the group and stimulus effects were not significant, the Group × Stimulus interaction was, $F(2, 9) = 11.4$, $p < .05$. Tests for the simple effect of stimulus revealed that the percentage of time spent in proximity to the two stimuli significantly differed only for those subjects that had been previously shocked in the presence of the imprinting stimulus, $F(1, 9) = 13.2$, $p < .05$.

The center panel of Figure 3 illustrates the behavior of the ducklings when they were confronted by a choice between the imprinting stimulus and the novel dissimilar stimulus before being given the 20-min. session of continuous exposure to the latter. As can be seen in this part of the figure, each group of ducklings showed a strong preference for the imprinting stimulus. An analysis of variance indicated that this difference in their approach to the two stimuli was statistically significant, $F(1, 9) = 297.7$, $p < .05$, the group differences and the Group × Stimulus interaction were not.

The ducklings' performance in the same choice situation after the 20-min. session of exposure to the novel dissimilar stimulus is illustrated in the right panel of Figure 3. Unlike their behavior during the initial choice test involving the novel dissimilar stimulus, the subjects previously shocked in the presence of the imprinting stimulus showed less of a tendency to approach and stay by it than they did for the novel one. No similar reversal of preference emerged in either of the two other groups of ducklings, and an analysis of variance indicated that there was a statistically significant Group × Stimulus interaction, $F(2, 9) = 37.2$, $p < .05$; the individual main effects were not significant. Tests for the simple effect of stimulus revealed that the percentages of time spent in proximity to the two stimuli differed significantly for each of the three groups, $F(1, 9) = 14.7$, $p < .05$.

In summary, the findings illustrated in Figure 3 indicate that the ducklings previously shocked in the presence of the imprinting stimulus showed a significantly reduced tendency to approach and stay by that stimulus when given a choice between it and either a novel similar stimulus or a novel dissimilar stimulus to which they have had 20 min. of continuous exposure. Neither the subjects previously shocked in the absence of the imprinting stimulus nor the subjects not previously shocked showed either of these effects.

Approach to the Novel Dissimilar Stimulus

Figure 4 illustrates the mean percentage of time the ducklings in each group spent in the quadrant closest to the novel dissimilar stimulus during the 20-min. session of continuous exposure to it. As revealed by this figure, the subjects previously shocked in either the presence or absence of the imprinting stimulus showed a greater tendency to approach and stay by the novel dissimilar stimulus than did the subjects not previously shocked. An analysis of variance for a completely randomized design (Winer, 1962) indicated that the difference between the groups was statistically reliable, $F(2, 9) = 8.4$, $p < .05$.

Discussion

The present pattern of results replicates both the enhanced following and reduced preference effects of aversive stimulation and also distinguishes between the conditions that are necessary and sufficient for their occurrence. On the basis of this distinction, it must be concluded that the two effects are based on different underlying processes.

Consider first the enhanced following effect: Ducklings that were recently shocked, regardless of whether in the presence or absence of the imprinting stimulus, showed a greater tendency to follow and stay by that stimulus when it was presented alone than did the nonshocked control subjects (see Figure 2). On this basis, one must conclude that the enhanced following effect is not dependent on an association between the presence of an imprinting stimulus and the administration of aversive stimulation. This conclusion is difficult to reconcile with Hess's and Eibl-Eibesfeldt's previously cited assertion that the enhanced following produced by such stimulation represents the formation of a stronger social attachment—it is not evident why, if this were the case, a stimulus whose absence had been associated with aversive stimulation would be as closely followed as a stimulus whose presence had been associated with it.

One alternative interpretation of the enhanced following effect that can readily account for the present findings is that it represents a motivational enhancement of a recently shocked subject's tendency to exhibit filial behavior toward an imprinting stimulus. This possibility is consistent both with the finding of Moltz and his collaborators suggesting that the extent to which a subject follows an imprinting stimulus is determined, in part, by the level of the bird's "anxiety" (Moltz & Rosenblum, 1958; Moltz et al., 1959) and with the notion that imprinting stimuli have "calming" properties (Hoffman & Ratner, 1973a; Hoffman, Ratner, Eiserer, & Grossman, 1974). According to this view, the ducklings in the present study that were recently shocked, in either the presence or absence of the imprinting stimulus, followed the calming object more closely than did the nonshocked controls because of a heightened level of anxiety.

The reduced preference effect, on the other hand, was evident only in the ducklings that had been shocked in the presence of the imprinting stimulus (see Figure 3) and, accordingly, appears to be dependent on the formation of an association between the imprinting stimulus and the aversive stimulation. This finding also poses a problem for Hess's and Eibl-Eibesfeldt's conclusion that aversive stimulation leads to the formation of a stronger imprinting bond and thereby questions their assertion that the development of such a bond is based on a unique learning process.

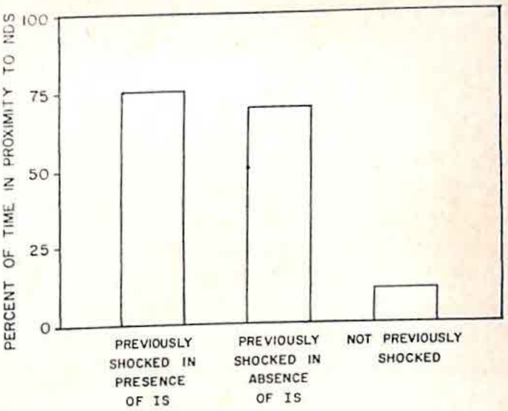

FIGURE 4. Approach to the novel dissimilar stimulus during the 20-min. session of continuous exposure to it. (Shocks were not administered during this session.)

The present findings concerning the reduced preference effect, however, are consistent with Hoffman and Ratner's "traditional" learning interpretation of the imprinting phenomenon. As was previously elucidated, the reduced preference effect, in terms of our model, reflects the consequence of associating an additional motivationally significant source of stimulation—in this case the electrical shock—with the static features that characterize the particular stimulus to which a bird is being imprinted. According to this view, one would expect the reduced preference effect to be limited to ducklings that had experienced aversive stimulation in the presence of the imprinting stimulus, as was indeed observed in the present study.

Following this line of reasoning, it appears that an association between an imprinting stimulus and aversive stimulation does not produce a stronger or weaker bond per se, but instead a bivalent attachment—one that incorporates all of the positive aspects of an ordinary bond but that is confounded by conditioned aversiveness. A stimulus associated with aversive stimulation apparently retains at least some of its comforting properties since the ducklings display an enhanced tendency to follow it when it is presented alone; as Harlow and Suomi

(1971) commented about the enhanced clinging behavior of infant monkeys raised with punitive mother surrogates, "Where else can a baby get protection?" (p. 541). In a testing situation in which the subject only has access to the mother surrogate, the obvious answer is "Nowhere." However, alternative sources of comfort and protection were provided by the novel stimuli in the choice tests of the present study, and, while there were substantial differences between the results obtained with the novel similar and novel dissimilar stimuli (see Figure 3), these differences can also be understood in terms of the reinforcement model of imprinting.

Consider first the results obtained with the novel similar stimulus (illustrated in the left panel of Figure 3). Being only slightly different than the imprinting stimulus, the conditioned positive reactions underlying the subjects' bond to the imprinting stimulus would be expected to generalize to the novel similar one to a great extent. This substantial generalization is indicated by the lack of a preference for either of the two stimuli by the subjects that were previously shocked in the absence of the imprinting stimulus and those that were not previously shocked. For the subjects that were previously shocked in the presence of the imprinting stimulus, however, both positive and negative reactions would be expected to generalize to the novel similar stimulus. If, as suggested by a number of investigators (e.g., Hoffman & Fleshler, 1963; Miller, 1959), approach behaviors generalize more broadly than do avoidance reactions along a given stimulus continuum, then the positive reactions associated with the imprinting stimulus would be expected to generalize to the novel similar stimulus to a greater extent than the negative ones. If they did, the reduced preference for the imprinting stimulus would represent the difference in the net attractiveness of the two stimuli.

The novel dissimilar stimulus, on the other hand, was very different from the imprinting stimulus, and little generalization (of either positive or negative reactions) would be expected to occur between them. Indeed, previous work (Hoffman, Ratner, & Eiserer, 1972) has indicated that the novel dissimilar stimulus used here is sufficiently different from the imprinting stimulus to initially elicit strong fear reactions, the characteristic response of birds to unfamiliar stimuli after the first day or so posthatch (Hess, 1962; Hoffman & Ratner, 1973a, 1973b; Ratner & Hoffman, 1974; Schaller & Emlem, 1962). Such fear reactions may have precluded any tendency to approach the novel dissimilar stimulus in the present experiment since the reduced preference effect was not evident in any of the groups of ducklings during the initial choice test between the imprinting stimulus and the novel dissimilar stimulus (see the center panel of Figure 3). During the subsequent session of exposure to the novel dissimilar stimulus, however, the fear reactions of the subjects that were previously shocked in either the presence or absence of the imprinting stimulus gradually abated (presumably as the novel stimulus became increasingly familiar; Hoffman & Ratner, 1973a; Ratner & Hoffman, 1974), and these ducklings began to approach the novel dissimilar stimulus (see Figure 4); but despite their near equal tendency to do so, the final choice test between the two stimuli again revealed that the reduced preference effect was restricted to the subjects that were previously shocked in the presence of the imprinting stimulus (see the right panel of Figure 3). According to the interpretation developed here, the choice to approach the novel dissimilar stimulus rather than the one to which they were initially imprinted reflects their behavior when confronted by two positive stimuli, one of which (i.e., the imprinting stimulus) was also somewhat aversive.

In summary, the results of Experiment 1 indicate that aversive stimulation has at least two distinguishable effects in the context of imprinting. The first of these—the enhanced following effect—appears to be based on a motivational increment in the subject's tendency to respond filially to a comforting, moving stimulus. The stimulus to which the subject has been imprinted can ordinarily fulfill this need, but if the stimulus itself was associated with the administration of the aversive stimulation, then a different stimulus that does not elicit fear (either because it is similar to the imprinting stimulus or because the subjects have had sufficient exposure to it) is preferred. The choice of a stimulus other than the one to which the subject is imprinted defines the second consequence of aversive stimulation in the context of imprinting—the reduced preference effect.

By being able to account for the present findings in terms of the classical conditioning principles of our reinforcement model of imprinting, the results of Experiment 1 provide support for both (a) Barrett's assertion that the effects of aversive stimulation in the context of imprinting are analogous to those occurring in the context of ordinary instances of learning and (b) a traditional conceptualization of the imprinting phenomenon itself.

This is not to say, however, that aversive stimulation might not facilitate the imprinting process in an indirect way; one would expect, for example, that the enhanced following produced by aversive stimulation would increase the subject's attention to the imprinting stimulus and hence facilitate the classical conditioning process presumed to underlie the development of familiarity with it. Regardless of whether it does, however, it is clear, especially considering the reduced preference effect, that aversive stimulation does not produce a stronger imprinting bond.

Besides clarifying the enhanced following and reduced preference effects, the results of Experiment 1 raise several additional questions concerning the effects of aversive stimulation in the context of imprinting. Consider, for example, the data presented in Figure 4. Relative to the nonshocked subjects, both groups of ducklings that received aversive stimulation during the prior imprinting sessions spent considerably more time in the proximity of the novel dissimilar stimulus during the 20-min. session of continuous exposure to it. While this difference might reflect the increased need for a calming stimulus that was postulated to underlie the enhanced following effect shown by these subjects, an additional process may have been operating. More specifically, there is considerable evidence that the administration of early stimulation (e.g., electrical shock, handling, etc.) to infant rats and mice reduces their adult emotionality (see Ambrose, 1969). Since reduced emotionality in older ducklings might well be reflected as a reduced fear of novel imprinting stimuli, an analogous *early stimulation effect* may have subserved, at least in part, the shocked subjects' greater approach to the novel dissimilar stimulus. This possibility was examined in a subsequent experiment of this series.

Another question bearing on the effects of aversive stimulation in the context of imprinting is what the effects of such stimulation would be if it was administered to *older* birds during exposure to a novel imprinting stimulus. The ducklings in the present experiment were only shocked during sessions of exposure to an imprinting stimulus that elicited filial behaviors; since older birds would initially react fearfully to an unfamiliar imprinting stimulus, one might expect that the introduction of aversive stimulation to a novel imprinting situation would have different behavioral consequences for older ducklings than those for the newly hatched subjects in the present study. Indeed, Hess has already demonstrated that the administration of such stimulation to older birds *reduces* the extent to which they follow a novel imprinting object in a nonchoice situation. In contrasting this reduction with the enhancement obtained with newly hatched subjects in the same situation, Hess concluded that, unlike imprinting soon after hatching, the formation of social attachments by older birds is based on a traditional process such as discriminative or associative learning.

Hoffman and Ratner's reinforcement model of imprinting, on the other hand, asserts that imprinting among both newly hatched and older birds only differs in that the fear of the older subjects must be habituated (as was done with the novel dissimilar stimulus in the present study) before the usual classical conditioning processes can operate. Unlike Hess, then, we consider imprinting among older birds to be based on the same learning processes that are postulated to be the basis of the phenomenon among newly hatched subjects. Experiment 2 of the present article focused on this issue by further examining the effects of administering aversive stimulation to older birds in a situation involving a novel imprinting stimulus and by determining whether these effects are comparable to those thus far obtained with newly hatched subjects. If the effects observed in older birds can be understood within the same theoretical framework that was developed in the first study, then the argument that imprinting among newly hatched and older birds is based on different processes would be called into question.

EXPERIMENT 2

As briefly discussed in Experiment 1, unless a precocial bird becomes familiar with a particular imprinting stimulus during the first day or so of posthatch, subsequent exposure to that stimulus initially elicits strong fear reactions rather than the filial behaviors that would otherwise characterize its response. If an older bird is given repeated or prolonged exposure to an unfamiliar imprinting stimulus, however, these novelty-induced fear reactions gradually subside, and the characteristic filial behaviors become increasingly evident (Hoffman & Ratner, 1973a; Hoffman et al., 1972; Ratner & Hoffman, 1974; Ratner, Note 1). The present experiment was designed to assess the effects of aversive stimulation on these behavioral changes.

While little research in the context of imprinting has focused on the effects of administering aversive stimulation to older ducklings, the major finding has been that such stimulation impairs the eventual filial behavior measured by following (Hess, 1964). This observation, however, was made under conditions in which the subjects were electrically shocked in the presence of an unfamiliar imprinting stimulus. Considering the importance of associative processes in producing the effects observed in Experiment 1, the question emerges as to whether a similar impairment would also occur if older subjects were shocked exclusively in the *absence* of an unfamiliar stimulus. Experiment 2 examined this possibility.

Method

Subjects and Apparatus

The subjects were 12 Khaki campbell ducklings hatched and reared in a fashion identical to that of Experiment 1. The experimental apparatus described in the first study was also used in Experiment 2.

Procedure

Upon removal from the incubator at 10–12 hr. posthatch, each duckling was fitted with a pair of wingbands, randomly assigned to one of three experimental groups ($n = 4$), and placed in its individual housing unit where it remained undisturbed for the next 4 days.

On Day 5 posthatch, each duckling was individually transferred to the subject compartment of the experimental apparatus. The imprinting stimulus was then continually presented for a 5-min. period, its path restricted to one-half the length of the stimulus compartment. During this initial test, the amount of time that the subjects spent in the quadrant closest to the stimulus was continually assessed and recorded.

Immediately after this approach test, the path of the imprinting stimulus was increased to the full length of the stimulus compartment and each duckling was afforded 20 min. of alternating 50-sec intervals of stimulus presentation and withdrawal. As in Experiment 1, the three groups of ducklings were (a) shocked in the presence or (b) absence of the imprinting stimulus, or (c) not shocked at all. The shock parameters were identical to those used in the first experiment, and each subject, regardless of whether it was to be actually shocked, was connected to the shock delivery system. Five additional sessions of such exposure were run over the next 2 days, and each session was immediately followed by a 5-min. approach test identical to the initial test given before the first session. Shocks were not administered to any of the subjects during these tests.

Results

Figure 5 illustrates the mean percentage of time that the ducklings in each of the three groups spent in the quadrant closest to the imprinting stimulus during the successive 5-min. approach tests. As can be seen in this figure, the rate at which the subjects began to approach the stimulus was dependent upon the condition of aversive stimulation that prevailed during the intervening 20-min. sessions. The subjects that were electrically shocked during the absence of the imprinting stimulus during these sessions began to approach the imprinting stimulus more rapidly than did the nonshocked controls.

In contrast, the ducklings that were shocked in the presence of the imprinting stimulus consistently showed less tendency to approach the stimulus than did the nonshocked subjects; indeed, the extent of their approach during the repeated tests showed practically no increase over the duration of the experiment. A repeated-measures analysis of variance (Winer, 1962) of the data on which Figure 5 is based indicate that the group effect, test effect, and Group×Test interaction were each statistically significant, $F(2, 9) = 12.9$, $p < .05$, for the group effect, $F(6, 54) = 9.72$, $p < .05$, for the test effect, and, $F(12, 54) = 2.88$, $p < .05$, for the interaction. In addition, paired-group comparisons using Duncan's Multiple Range Test (Winer, 1962) revealed that the performance of each of the three groups differed significantly from that of the other two, ($p < .05$).

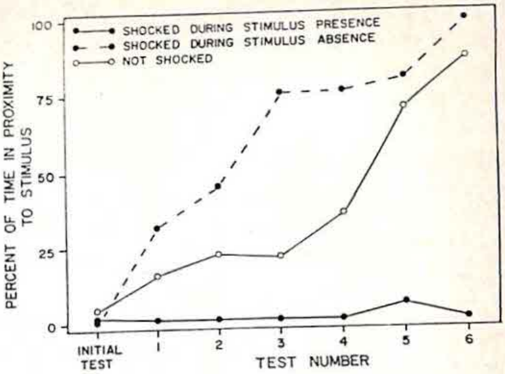

FIGURE 5. Approach to the novel moving stimulus by the nonimprinted older ducklings during the repeated 5-min. tests. (The initial test was given prior to the first 20-min. session of exposure to the stimulus. Shocks were not administered during these tests.)

Discussion

The results of the present study replicate both the gradual development of approach behavior that occurs when an older precocial bird is given repeated exposure to an unfamiliar imprinting stimulus and Hess's finding that the administration of aversive stimulation in the presence of such a stimulus retards this development. During the early tests, the subjects in each of the three groups showed a strong tendency to avoid the area of the subject compartment that was in proximity to the imprinting stimulus. While this avoidance behavior gradually waned and was replaced by approach to the stimulus for the nonshocked ducklings and the ducklings shocked during stimulus absence, the subjects that were shocked in the presence of the imprinting stimulus showed little decrease in their initial tendency to avoid it. Since this retarded development of approach was evident only when aversive stimulation was associated with the presence of the imprinting stimulus, it appears, as did the reduced preference effect in Experiment 1, to be based on an associative process.

In contrast to the behavior of the ducklings that were shocked in the presence of the imprinting stimulus, the present findings also demonstrate that ducklings shocked in the absence of an initially fear-eliciting stimulus display an *enhanced* rather than retarded development of approach. While this enhancement may have been subserved by the imprinting stimulus having become a safety signal (i.e., a cue for the nonoccurrence of aversive stimulation), it may also represent the increased need for a calming imprinting stimulus that is engendered by administering aversive stimulation in the imprinting situation. Presumably, the manifestation of this increased need is masked by the maintenance of the initial avoidance tendency when shock is associated with the presence of the stimulus.

Regardless of the basis of the enhanced approach shown by the ducklings that were shocked during periods of stimulus absence, however, the results of the present experiment, as of the first study, point to the importance of associative processes in determining the effects of aversive stimulation in the context of imprinting. If shock is associated with an imprinting stimulus that *does not* elicit novelty-induced fear (as in Experiment 1), ducklings, despite their tendency to follow the stimulus more closely than they otherwise would, show a reduced tendency to approach the imprinting stimulus when a suitable alternative is available. Paralleling this effect, if shock is associated with an imprinting stimulus that *does* elicit novelty-induced fear (as in the present study), ducklings maintain (at least for the duration of Experiment 2) their initial tendency to avoid the stimulus even in a non-choice situation.

In contrast, when shock is exclusively administered in the absence of an imprinting stimulus, a duckling's tendency to approach and follow the stimulus is enhanced regardless of whether the stimulus elicits novelty-induced fear. Apparently, aversive stimulation does not necessarily enhance or impair the effectiveness of the imprinting experience nor does it necessarily produce a stronger or weaker social attachment in either a fearless young bird or fearful older one; instead, its effects are determined by the conditions under which the stimulation is administered and the circumstances under which the effects are assessed. Taken together, the results of the first two experiments suggest that, while these consequences can interact with each other in a complex fashion, they can nonetheless be understood, for both newly hatched and older birds, in terms of the effects engendered by aversive stimulation in more traditional learning situations.

EXPERIMENT 3

The results of Experiment 1 indicate that the administration of electrical shocks to newly hatched ducklings in either the presence or absence of an imprinting stimulus facilitates their subsequent tendency to approach an initially fear-eliciting novel stimulus (see Figure 4). Since the subjects that showed this effect were previously shocked in the same apparatus in which they were exposed to the novel stimulus, the facilitation of their approach tendency may have been attributable (as was their enhanced following of the original imprinting objec to an increased need for the comfort provided by a moving stimulus.

As discussed earlier, however, an additional process contributing to this facilitation may have been that the administration of electrical shock to the newly hatched ducklings decreased their subsequent tendency to react fearfully to novel stimuli. Although not yet documented for precocial birds, there is considerable evidence that if administered at an appropriate early age, several forms of stimulation—for example, electrical shock (Levine, Chevalier, & Korchin, 1956; Kline & Denenberg, Note 2), handling (Denenberg, 1962, 1964, 1967; Levine, 1968), and abrupt changes in temperature (Schaefer, 1968)—have significant impact on the ontogeny of emotional behavior in rats and mice. Since work in this area has generally indicated that these animals show reduced emotionality in fear-eliciting situations as a consequence of experiencing such stimulation at an early age, Denenberg (1964) has suggested that there is an inverse monotonic relationship between the amount of stimulation experienced in infancy and subsequent "emotional reactivity" (often measured by assessing the subject's behavior in a novel open-field setting). Experiment 3 of the present series sought to determine whether newly hatched ducklings are susceptible to a corresponding early stimulation effect by assessing their adaptation to a novel environment as a function of the age at which they were previously given electrical shock.

The documentation of an early stimulation effect in ducklings, besides indicating the generality of the relationship between infantile stimulation and the ontogeny of emotional behavior, would also provide support for the hypothesis that the facilitated approach to the novel dissimilar stimulus observed in the first experiment was subserved, at least in part, by either a decreased or more rapidly habituated tendency for the shocked subjects to react fearfully to the novel stimulus. Since, as discussed earlier, novelty-induced fear reactions are presumed to prevent an older duckling from exhibiting filial behavior toward an unfamiliar imprinting stimulus, a reduction in these reactions due to early stimulation would be expected to be accompanied by an increased tendency to approach a novel stimulus. In order to examine this notion directly, the procedure of Experiment 3 also entailed exposing the previously stimulated ducklings to an unfamiliar moving object after the assessment of their emotional reactivity in the open-field setting; if the preceding analysis is correct, the ducklings that show reduced emotionality in the open field should also begin to approach the novel stimulus more rapidly than subjects that do not.

Method

Subjects and Apparatus

The subjects were 15 Khaki campbell ducklings hatched and reared in a fashion identical to that of Experiment 1. The experimental apparatus described in Experiment 1 was also used in the present study with the following modification: A microphone was suspended into the center of the subject compartment in order to detect the ducklings' distress vocalizations during the open-field test. In the duckling, the distress call has a relatively restricted frequency range (approximately 3,000–4,000 Hz) and is relatively intense (Collias & Joos, 1953); hence, it can be readily detected and distinguished from ambient background noises if the input to a sensitive voice-operated relay is selectively filtered. In the present experiment, this was accomplished by channeling the output of the microphone into a specially constructed voice key that has been described in detail elsewhere (see Hoffman et al., 1972).

Procedure

Phase 1: Stimulation. As in the previous studies of this series, the ducklings were transferred from the incubator to their individual housing units at 10–12 hr. posthatch and, at the same time, fitted with wingbands and randomly assigned to one of three groups ($n = 5$): early stimulation, late stimulation, or no stimulation.

Electrical shock was administered to the subjects in the early stimulation group during four 20-min. sessions that were evenly distributed over Days 2 and 3 posthatch. In order to do so, wires from an Applegate Constant-Current Stimulator located in the room in which the subjects were housed were lowered into a subject's housing unit and attached to the bird's wingbands. A single shock (1.0 mA intensity, 300 msec duration) was administered every 20 sec throughout the course of each session. The ducklings in the other two groups, while not shocked on Days 2 and 3, were nonetheless connected to the stimulator for corresponding periods of time.

On Days 5 and 6 posthatch, the subjects in the late stimulation group were electrically shocked in a fashion identical to that previously employed with the ducklings in the early stimulation group. Also identical to the experimental procedures of Days 2 and 3, the ducklings in the early and no-stimulation groups, not shocked on Days 5 and 6, were regularly connected to the stimulator.

Phase 2: Open-field test. On the seventh day posthatch, the ducklings in each of the three groups were individually placed in the subject compartment of the experimental apparatus, and their tendency to emit distress vocalizations in the novel environment was continually monitored for a 15-min. period. While distress calls are emitted by immature ducklings in a variety of aversive situations (Hafez, 1962; Hoffman, Stratton, Newby, & Barrett, 1970; Sluckin, 1965), recent work by Candland and Nagy (1969) and Montevecchi, Gallup, and Dunlap (1973) has indicated that when placed in a novel setting, a bird's distress calling is often precluded by another characteristic fear reaction—freezing in a crouched position; accordingly, in addition to monitoring distress calls, the procedure of Experiment 3 also entailed an assessment of the extent to which the ducklings in each of the three groups exhibited this behavior. In order to do so, an experimenter, who did not know which groups the subjects were in, observed each duckling while it was in the open field (via the closed-circuit television system) and recorded the durations of instances in which the ducklings were crouched against a wall of the open field. Shocks were not administered to any of the subjects during the open-field test.

Phase 3: Exposure to the novel stimulus. Beginning on the latter part of the seventh day posthatch and continuing through the ninth day, each duckling was individually given six 20-min. sessions of continuous exposure to the imprinting stimulus described in Experiment 1. Shocks were not administered during these sessions, and the path of the stimulus encompassed the full length of the stimulus compartment.

Immediately preceding its initial exposure to the imprinting stimulus and immediately following each session thereafter, each duckling was individually given a 5-min. test designed to assess its approach to the stimulus. In order to do so, the path of the imprinting stimulus was restricted to one-half the length of the stimulus compartment, and the duckling's location was continually monitored by the photocell arrangement described in Experiment 1 and recorded on the Esterline-Angus Event Recorder.

Results

During the course of the experiment, a duckling in the early stimulation group that was not eating properly died. The data generated by this subject was omitted from the subsequent analysis.

Open-Field Test

When placed in the subject compartment of the experimental apparatus for the first time, each duckling either ran about in circles emitting distress calls or silently crouched against a wall of the compartment. Observation of the ducklings over the closed-circuit television system supported Montevecchi et al.'s suggestion that these behaviors are mutually exclusive—indeed, there were only two brief instances in which a bird was observed to simultaneously distress call and crouch. On the basis of this exclusiveness, it is appropriate to combine the percentages of time that the ducklings exhibited one or the other of these behaviors in order to derive a single index of their emotional reaction to the open field.

This was done in Figure 6, which illustrated the percentages of time that the ducklings in each of the three groups either emitted distress calls or crouched as a function of successive 3-min. periods of the 15-min. open-field test. As revealed in this figure, only the ducklings in the early stimulation group stopped distress calling and crouching by the end of the 15-min. test despite the fact that their initial tendency to have these reactions did not differ from that of the subjects in the other groups. A repeated-measures analysis of variance indicated that the trends depicted in Figure 6 are statistically reliable, $F(8,\ 44) = 2.83$, $p < .05$, for the Group × Period interaction, and a test for the simple main effect of period revealed that the decline in fear reactions of the subjects in the early stimulation group was significant, $F(4,\ 44) = 15.63$, $p < .05$.

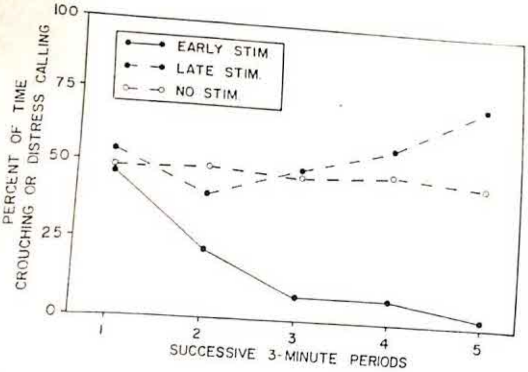

FIGURE 6. The percentage of time the ducklings spent crouching or distress calling during successive 3-min. periods of the open-field test.

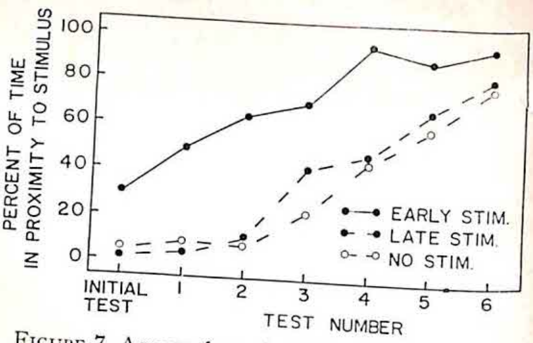

FIGURE 7. Approach to the novel moving stimulus by the nonimprinted older ducklings during the repeated 5-min. tests. (The initial test was given before the first 20-min. session of exposure to the stimulus.)

Approach to the Novel Stimulus

Figure 7 illustrates the mean percentage of time that the subjects in each of the three groups were in the quadrant closest to the imprinting stimulus during the 5-min. approach tests administered on Days 7 through 9 posthatch. As can be seen in this figure, the ducklings in each of the three groups showed an increasing tendency to approach and stay by the stimulus; the ducklings that had previously experienced early stimulation, however, developed a strong approach tendency more rapidly than did either the late or no-stimulation ducklings. A repeated-measures analysis of variance indicated that these trends are statistically reliable; the difference between the groups and the increase in approach across tests were both found to be significant: For the group effect, $F(2,\ 11) = 4.93$, $p < .05$, and for the test effect, $F(6,\ 66) = 17.90$, $p < .05$.

GENERAL DISCUSSION

The present findings demonstrate that ducklings that are electrically shocked on the second and third days posthatch subsequently cease exhibiting fear reactions (such as distress calls and crouching) in a novel environment more rapidly than do ducklings that are either shocked at an older age or not shocked at all. This early stimulation effect parallels that previously observed in rats and mice and thereby reveals that infantile experience is an important determinant of subsequent emotionality in precocial birds as well as in mammalian species.

This conclusion is consistent with the hypothesis that the facilitated approach to the novel dissimilar stimulus shown by the shocked subjects in Experiment 1 was due, at least in part, to a reduced tendency for them to react fearfully of it. Since the ducklings in the first study were shocked at the same age as the subjects in the early stimulation group of the present experiment, it seems likely that their aversive reactions to the presumably fear-eleciting stimulus dissipated more rapidly than that of the nonshocked controls, thereby allowing the more rapid expression of the otherwise suppressed filial behavior.

Further evidence for this assertion is provided by the finding that the subjects that showed reduced emotionality in the open field during the present experiment also began to approach a novel imprinting stimulus more rapidly than did the other subjects. This was so despite the fact that the electrical shocks that produced the present effects were administered to the ducklings in their home containers rather than in the experimental apparatus (as was done in Experiment 1); since, under these conditions, the testing situation would not have become differentially aversive for the shocked subjects, it is not possible (as it was in the first study) that the facilitated approach observed in the present experiment represented an increased need for a comforting moving object.

This is not to say, however, that such an increased need could not have contributed to the facilitated approach observed in Experiment 1. Indeed, comparison of the extent of approach after a single 20-min. session of exposure to the novel stimuli of the two experiments (see Figures 4 and 7) indicates that the magnitude of the facilitation observed in the first study was greater than that observed in the present experiment. Hence, it is possible that some process in addition to a decrease in emotional reactivity in fear-eliciting situations contributed to the facilitated approach to the novel dissimilar stimulus that was observed in Experiment 1.

Looking at the other side of the same coin, the present findings also suggest the possibility that decreased emotional reactivity—in addition to the suggested increased need for a comforting stimulus—contributed to the enhanced following of the original imprinting object observed in Experiment 1 (see Figure 2). This possibility seems unlikely, though, since the changes in emotional reactivity produced by early stimulation have been generally noted, as discussed previously, in fear-eliciting situations. A newly hatched duckling's behavior, in contrast, does not appear to be fear mediated at all. Indeed, the young duckling's immediate response—even upon first exposure to an imprinting stimulus—is typically characterized by both strong and persistent positive reactions (Hoffman et al., 1970) and a willingness to work to keep the stimulus in view (Bateson & Reese, 1968; Ratner & Hoffman, 1974). Accordingly, while the facilitated approach to the novel dissimilar stimulus shown by the shocked subjects in Experiment 1 appears to reflect reduced emotionality as well as an increased need for comfort, the enhanced following of the original imprinting object shown by the same subjects appears to be based just on the latter.

The demonstration of an early stimulation effect in the present study suggests that the administration of aversive stimulation soon after ducklings hatch can be a useful procedure in furthering our understanding of fear-mediated processes in the context of imprinting. The use of early shock in the present study, for example, provided support for the notion that an older duckling's tendency to react filially toward an unfamiliar imprinting stimulus is suppressed by novelty-induced fear—approach to such a stimulus developed most rapidly in subjects that showed a reduced tendency to exhibit fear reaction in a novel environment. Another example of what seems to be a fear-mediated process in the context of imprinting is the aggression that, under appropriate conditions, an older duckling directs at an unfamiliar stimulus (Hoffman et al., 1974). If this behavior is indeed fear mediated, then one would expect, on the basis of the present findings, that the administration of electrical shock to a newly hatched bird would reduce the amount of aggression it subsequently directs at such a stimulus. Further research, of course, is needed before one can assert this interesting possibility with any degree of confidence.

Besides being useful in the context of imprinting, the demonstration of an early stimulation effect in ducklings points to an efficient means of conducting research designed to analyze the mechanism of the effect itself. In the past, such work has been typically done with rats and mice (e.g., see Denenberg, 1967; Schaefer, 1968); where it is possible, the future use of more rapidly maturing ducklings would shorten the time required to produce and study the effect. Indeed, while the early stimulation effect was demonstrated within a week after the subjects of the present experiment hatched, a similar demonstration using rats or mice typically entails maintaining the subjects for several weeks or months.

SUMMARY AND CONCLUSIONS

Past research has revealed that the administration of aversive stimulation to a newly hatched precocial bird while it is being exposed to an imprinting stimulus has two notable behavioral consequences: (a) It enhances the subject's tendency to follow the stimulus if the stimulus is presented alone (Hess, 1959; Kovach & Hess, 1963), and (b) it reduces the subject's preference for the stimulus when the choice is given to approach either it or a novel but similar one (Barrett, 1972).

In addition to replicating these consequences, the present series of experiments also examined and compared the behavioral effects of administering aversive stimulation during the withdrawal periods of a repeatedly presented and withdrawn imprinting stimulus. The use of this procedure generated a pattern of results (see Experiment 1) suggesting that the enhanced following engendered by aversive stimulation simply represents a motivational increment in the subject's tendency to follow and stay by a comforting moving object, while the reduced preference for a stimulus that has been associated with the aversive stimulation represents a conditioning phenomenon.

In addition, the impairment or facilitation of approach to a fear-eliciting imprinting stimulus when older birds were subjected to aversive stimulation in either the presence or absence of the stimulus (Experiment 2) was also interpretable in terms of such stimulation having both motivational and associative effects in the context of imprinting. By demonstrating and clearly distinguishing between these effects, the results of Experiments 1 and 2 question the notion that aversive stimulation increases the effectiveness of the imprinting experience and produces a stronger social bond—a conclusion that has led some investigators (e.g., Eibl-Eibesfeldt, 1970; Hess, 1964) to assume that imprinting represents a unique phenomenon. Instead, the results obtained in the first two studies indicate that the effects of aversive stimulation are determined by the conditions under which the stimulation is administered and the circumstances under which they are assessed. While these effects were found to interact with each other in a complex fashion, it was nonetheless possible to understand them in terms of the effects engendered by aversive stimulation in more usual instances of learning. Indeed, Hoffman and Ratner's (1973a) recently proposed model of imprinting, which assumes that imprinting is based on simple classical conditioning processes, can readily incorporate all of the observations made in Experiments 1 and 2.

The present research also revealed a third effect of aversive stimulation in the context of imprinting—namely, an early stimulation effect. Ducklings that were electrically shocked in Days 2 and 3 posthatch subsequently acclimated to a novel open-field setting and began to approach a novel, fear-eliciting stimulus more rapidly than did subjects that were either shocked at an older age or not shocked at all (Experiment 3). While this early stimulation effect had not been noted in prior imprinting studies, it appeared, in the present research, to interact often with the motivational and associative effects of aversive stimulation. Its demonstration, additionally, suggests that the inverse relationship between infantile stimulation and subsequent emotional reactivity in fear-eliciting situations that has been reported for rats and mice is also characteristic of immature ducklings. It was noted that the future use of this developmental effect of aversive stimulation might further our understanding of fear-mediated processes in the context of imprinting as well as provide an efficient means of studying the effect in its own right.

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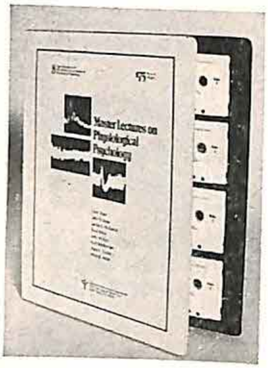

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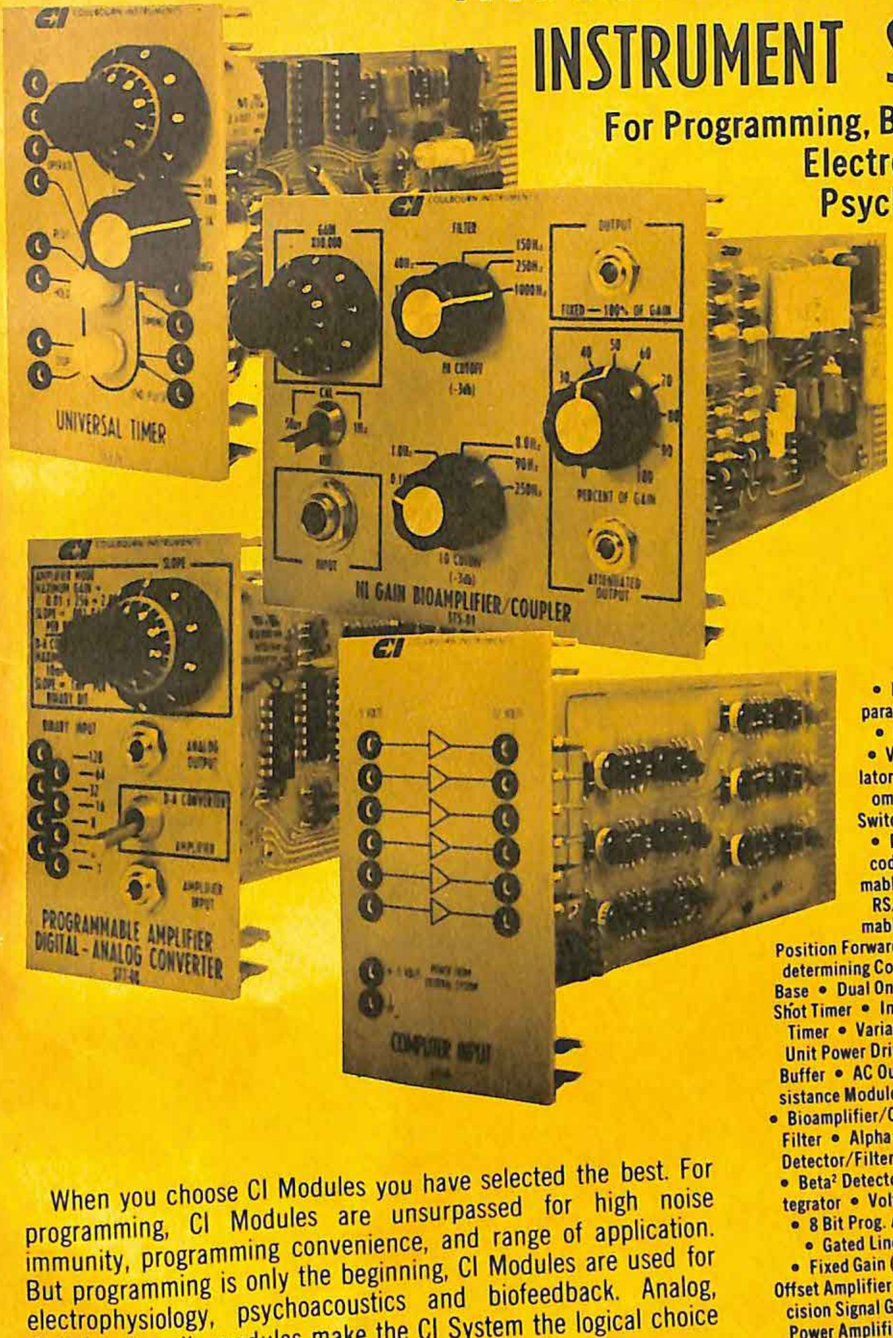

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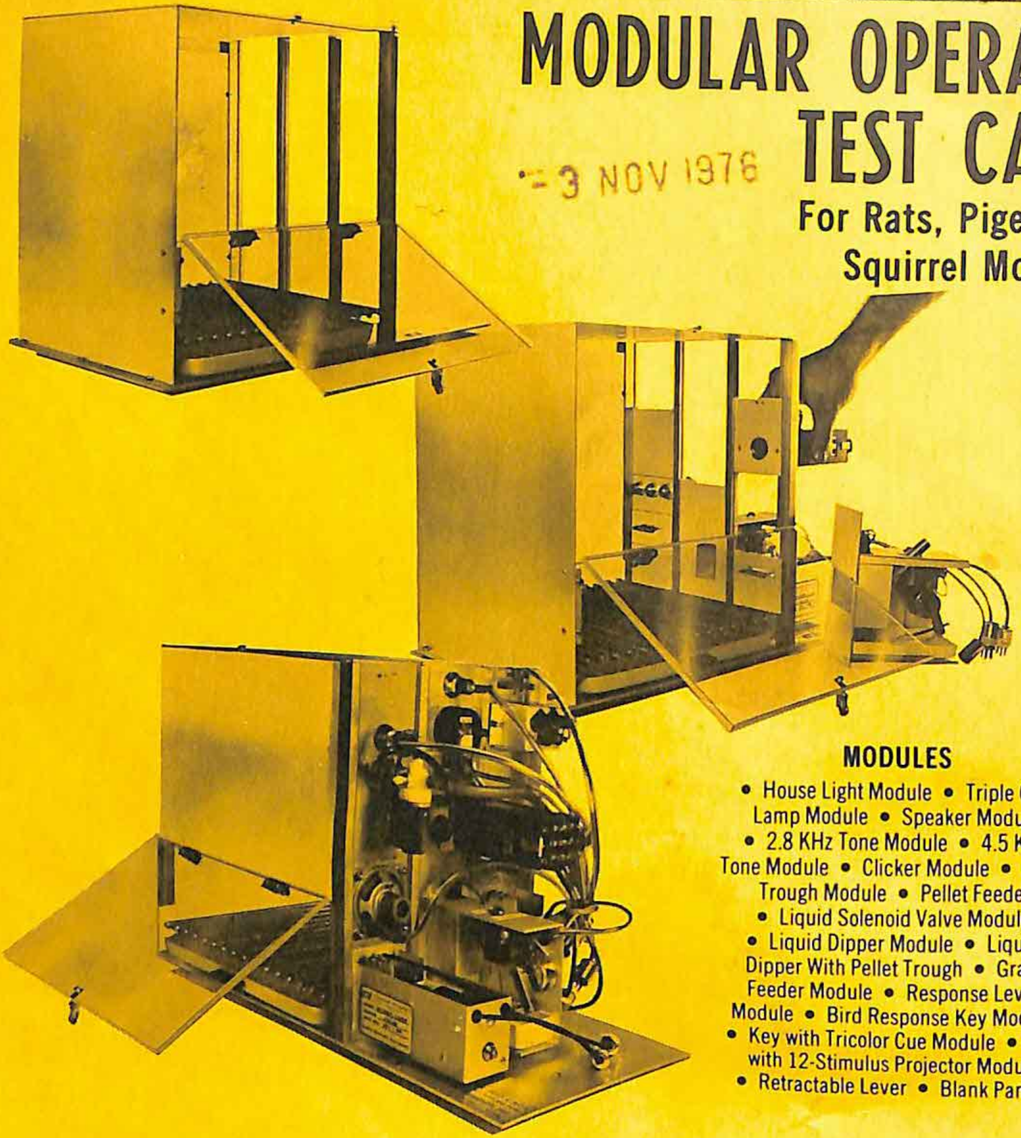

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Natural Concepts in Pigeons

R. J. Herrnstein, Donald H. Loveland, and Cynthia Cable
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Pigeons learned to discriminate pictures of trees, bodies of water, or a particular person in three separate experiments. Pictures being seen for the first time were discriminated almost as well as pictures seen in training. The pigeons in each experiment showed similar patterns of errors and correct discrimination.

The list of stimuli used in typical discrimination-learning experiments may be long, but it is characterized by a definiteness that sets it apart from the list of stimuli animals confront in nature. In nature, open-ended variability is the rule rather than reproducibility or definiteness. Squirrels may learn where the acorns are, but the acorns will vary in size, shape, and color, and so will the oak trees. Mice may learn to be wary of houses and yards with cats in them, even though cats look different. The variation within naturally occurring stimulus classes typically defies our capacity for physical definition, unlike the 1,000-Hz tones or 465-mμ lights of a psychological experiment. It is doubtful whether anyone can yet instruct a machine to identify acorns or cats let alone the stimulus classes that form the basis of human language, such as *chair*, *house*, or *mama*.

There seem to be two sorts of reasons why experimentation fails to match nature better—the practical and the theoretical. Practically speaking, it is far easier to present a rat with repeated bursts of a 1,000-Hz tone than to reproduce the limitlessly variable sounds it responds to in nature—the peeps of other rats, for example. Similarly, it is easier to present pigeons with a red circle of specified diameter than to take hundreds or thousands of pictures of the roughly spherical grains it eats.

The practical bias toward reproducibility is bolstered by theory. Natural stimulus classes are held to be merely imperfect analogues of the artificial stimulus classes of the laboratory. On the origin of stimulus classes, theorists as different in other respects as Hull (1943), Skinner (1953), and Bruner (Bruner, Goodnow, & Austin, 1956) fall mainly in the classical empirical tradition. Stimulus classes supposedly arise in the process of induction. For reinforcement theorists like Hull and Skinner, reinforcement produces the induction; for cogni-

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tivists like Bruner, the induction is taken as given. Either way, the creature sees, for example, a series of acorns and nonacorns which supposedly yields the defining parameters of acorns in general. Some set of values on a collection of visual variables marks off the region of acorns.

It is hard, and expensive, to study discrimination learning with the open-ended series of instances found in nature. It has been easier to assume that the *process* of discrimination is separable from the things being discriminated, even though there is reason to examine the possibility that, at least sometimes, the two are inseparably linked. But that possibility seems to require bringing natural discrimination into the laboratory. An approximation to such a natural discrimination procedure was reported by Herrnstein and Loveland (1964) and has since been replicated by others (e.g. Malott and Siddall, 1972). Pigeons were trained to peck a key only when they saw pictures containing people. They learned the discrimination rapidly and well, responding differentially to pictures seen for the first time. The essential feature of a natural discrimination—which is the ability to cope with natural ranges of variation—was at least approached. The present experiments extend the earlier findings by using other classes of stimuli.

METHOD

Subjects

Eleven male pigeons at about 80% ad-lib weight were used in three experiments. Three pigeons were homers (51H, 56H, and 63H); 8 were white Carneaux (7C, 8C, 24C, 44C, 45C, 91C, and 244C). Each pigeon worked in only one of the three present experiments. They all had previous experience in similar experiments with stimuli other than the ones reported here. Since there was no apparent differential effect of the earlier training on the present data, nothing more will be said about them.

Apparatus

A standard pigeon chamber was adapted for the projection of 35-mm slides by a Kodak Carousel projector. In addition to the standard pigeon key and feeder, the front wall of the chamber contained a rectangular screen 1¾ in. (.044 m) high × 2½ in. (.064 m) wide through which the slides were projected. The screen was in the center of the front wall, at the same height as the key but about 2 in. (.051 m) to its right. It took a force of about 15 g to operate the key. The feeder presented food for the 2.5 sec at a time, during which the key was darkened. A masking noise sounded continuously, and every peck of the key caused an audible click. An electromechanical circuit did the programming and recording for the early sessions, but then control was shifted to a PDP 9/T computer (Digital Equipment Corporation). All the data reported here came from the computer-run phase.

Procedure

During an experimental session, 80 slides were successively projected on the screen. The average duration of each presentation was 30 sec, varying irregularly from 10 to 90 sec. A 10-sec interval separated successive slides, during which the chamber was blacked out completely. The key was illuminated with a white light that came on 5 sec after the onset of a picture and stayed on (except during feeder operations) until the picture went off. Pecking counted only when the key was lit.

Each set of 80 pictures comprised 40 ± 5 examples of some object (e.g., trees, see below) and 40 ± 5 nonexamples. These are called positive and negative stimuli, respectively. The order of positive and negative stimuli was random and rescrambled for most sessions. That is to say, the pigeons had no opportunity to learn a given sequence of positive and negative stimuli, since most sessions presented new orders of pictures, even when the pictures themselves had been presented before.

In the presence of positive stimuli, pecking was reinforced on a variable-interval schedule with an average of 30 sec and a range from 3 to 90 sec. The schedule of reinforcement was independent of the schedule of stimulus durations even though their parameters were similar. The schedules permitted anywhere from zero to three reinforcements for a single positive stimulus. In the presence of negative stimuli, pecking earned no food. Moreover, a negative stimulus could not be terminated within 10 sec of a peck. This penalty for negative-stimulus pecking was superimposed on the basic schedule of stimulus duration.

The three experiments were identical except for the stimulus classes involved. In Experiment T, the positive class comprised pictures containing one or more trees or any part of a tree. In Experiment W, it was pictures containing water. In Experiment P, it was pictures containinng all or part of a particular person, a women in her late twenties who consented to having a photographer follow her from time to time over a period of about a year, taking posed and unposed pictures.

In all three experiments, the photographer (the third author) used a Nikon 35-mm single-lens reflex camera with a variety of lenses to take pictures during all of New England's four seasons. Experiment T (trees) employed 1,840 different pictures, of which half were positive and half negative. The trees were of virtually every description and variety found locally and were photographed from near or far, unobstructed and partially obstructed. For positive stimuli, we attempted to capture the full range of scenes considered to contain a tree or any part thereof, but not necessarily photographing the scene as if the tree were the center of attention. The sole restriction was that none of the 1,840 pictures contained shrubs or bushes. We chose to solve the problem of ambiguity here by avoiding it. The negative stimuli were comparable to the positive in every respect except for the presence or absence of trees, as far as we could tell.

Experiment W (water) used 1,760 different pictures, of which half were positive and half negative. Water meant anything from an aerial view of the Atlantic Ocean to obscure, small puddles. No pictures contained visible drops of water, as would be seen, for example, in rain. The negative stimuli often mimicked the more obvious visual properties of water—its shininess, smoothness, blueness, and so on. A number of pictures contained snow or ice. If these contained no liquid water, they were designated negative.

Experiment P (specific person) used 1,600 different pictures, of which half were positive and half negative. As before, every attempt was made to capture the person in the full range of settings—indoors or outdoors, near or far, front or rear, clear or obscure, alone or with other people.

All pictures were in color but of varying brightness and contrast. Many pictures served in more than one experiment, often switching status as negative or positive but not always. The pictures were *not* composed to draw attention to the relevant features. That is to say, many positive stimuli had the critical element off center, small, or distant, and therefore were easily overlooked by casual human observers. The decision about the contents of each picture was usually made by the photographer, occasionally with the help of one or both of the two other authors.

Each experiment lasted from 120 to 131 daily sessions. For most sessions, the 80 pictures were drawn randomly from a pool of about 500–700 (half positive, half negative). Consequently, some pictures were being seen for the first time, and others were repeaters, with the proportion shifting toward repeaters as the experiment progressed. On certain sessions, however, only new pictures were presented, and it is the data from these that have been analyzed here. Unless otherwise stated under Results, these pictures were distinct in no way other than being shown for the first time. No effort was made to improve learning by using easier pictures early in training or by any other explicit sequence.

RESULTS

Discrimination

Data from three experiments are presented together to aid comparison. Each session yielded a rate of pecking for each of the 80 pictures. Since the rate of pecking may be associated with the prior occurrence of reinforcement during the 10–90 sec that a picture is presented, the responding during a given positive stimulus after the first reinforcement is suspect. Figure 1 consequently shows the "corrected" rate of responding during positive stimuli, which means just the responding prior to the first reinforcement for each positive stimulus. During negative stimuli, a different need for correction existed. Negative stimuli cannot end within 10 sec of a peck. This penalty for pecking can artifactually depress the apparent rate of pecking, for it keeps the stimulus on until the response rates gets low. The corrected rate of negative-stimulus responding is for the responding during the scheduled 10–90 sec of presentation, that is, prior to the imposing of any penalty for responding.

The data in Figure 1 show 1 out of 10 consecutive sessions using only new pictures (i.e., 800 new pictures per experiment), on which most of this analysis of results is based for nine of the 11 subjects. These 10 sessions were run after about 75 sessions using pictures from the general pool. For all pigeons, including the two omitted in Figure 1, the positive pictures occasioned higher average rates of responding than the negative. Each pigeon also erred occasionally, responding slowly to positive stimuli or rapidly to negative. Beyond these generalities, not much obvious uniformity is displayed here by the subjects in each experiment. The absolute rates of responding varied widely, as shown by the adjustments in the scale of the y axis. Taking the subjects individually, rates of responding drifted up, down, or otherwise fluctuated during the session.

To assess statistical significance, the Mann–Whitney U test appears to be appropriately conservative (Siegel, 1956). If discrimination were perfect, the 40 highest corrected rates of responding would be to the positive stimuli, whereas the 40 lowest would be to the negative (assuming a 40/40 split). The absence of discrimination would result in a random mixture of positive and

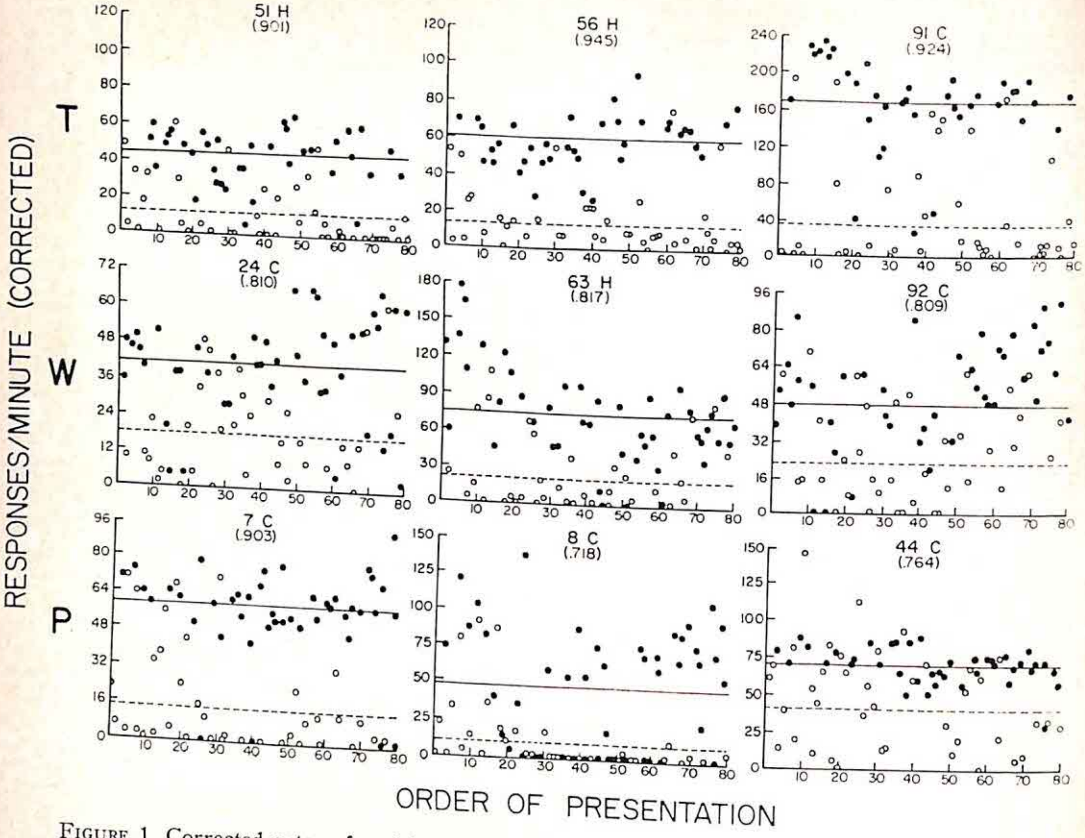

FIGURE 1. Corrected rates of pecking in the presence of each picture during a representative session using only new pictures, plotted against the order in which the pictures were shown. (Each coordinate axis is for one subject; each row for one of the three experiments. Filled circles are for positive stimuli; open circles, for negative stimuli. The solid horizontal line averages the filled circles; the dashed line averages the open ones. The decimals in parentheses are values of an index of discrimination, see text.)

negative stimuli throughout the list of ranks. *U*, which measures the degree of nonrandomness in the obtained ranking, confirmed the presence of discrimination in all nine cases in Figure 1. Discrimination was weakest for 8C, but the probability of drawing the obtained rankings by chance was nevertheless well below .01 even in this case. For the sessions shown in Figure 1, discrimination was strongest for 56H, where the associated probability was below .000,001.

Altogether, 108 sessions of data for new pictures were obtained for the 11 pigeons during the 10 days of tests (not 110, for there were two data-recording failures). The *U* test failed to achieve, or marginally achieved, a significance level of .05 five times, spread over the three experiments. The remaining 103 sessions were well, often far, into the range of statistical significance. Combining the tests of significance produced probability values so infinitesimally small that they are usually not tabled.

Although the *U* test establishes nonrandom ranking, it is not conveniently interpreted as a measure of the precise degree of nonrandomness. For that purpose, we divided the value of *U* for each session by the product of the numbers of positive and negative pictures to get an index ρ, a proportion that estimates the probability that the rank for the responding to a positive stimulus is above that to a negative stimulus (Bamber, 1975; Bradley, 1968). Bamber (1975) has shown that this quantity equals the area below a receiver operating characteristic graph whose coordinates are the probability of a positive instance being above a given rank and the probability of a negative instance being above that rank. When discrimination is perfect, ρ should be 1.0; when it is absent, it should be .5. Figure 2 shows the obtained values of ρ for the 10 sessions of testing with new pictures.

Taking the median ρ values (see Figure 2) as the measure of discrimination, pictures containing trees have about a 90% chance of eliciting higher pecking rates than pictures without trees for three of the four pigeons in Experiment T. Pigeon 244C, the fourth pigeon, had a median ρ of .745. The average of the four medians was .853. This is higher than the average of the medians in Experiment W, which was .790, or the average of the medians in Experiment P, which was also .790. Experiment T thus produced a slightly more accurate discrimination than the other two, but all three experiments demonstrated a concept involving a class of natural objects (i.e., pictures of those objects). The general level of discrimination in Figure 2 was no different from that shown in the training sessions using a mixture of old and new stimuli.

Comparisons across the three experiments are not like comparisons of discriminability in experiments using fixed stimuli in which a poor score means a poor discrimination. Here, the degree of discriminability must reflect to some extent the kind of exemplars we choose for a class. A poor score with obscure pictures does not signify that the discrimination has been poorly learned. We cannot, therefore, infer that trees are even slightly more discriminable than the other two classes, for the difference may have been in the selection of instances.

Figure 2 contains bands of cross-hatching on each coordinate. These show the region[1] of values of ρ that would have been obtained if the Mann-Whitney U had been statistically insignificant at the .05 level. Values of ρ ±.1 of .5 indicate insignificant nonrandomness in the rank order of rates of responding. As noted earlier, this occurred no more than five times in the 108 tests.

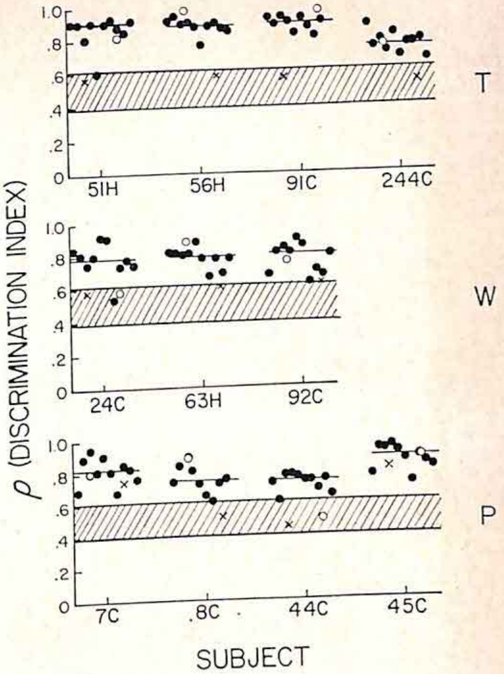

FIGURE 2. Discrimination index, estimating the probability that a positive stimulus will be ranked higher than a negative stimulus. (Each filled point gives the value of ρ for a single session using new pictures. The short horizontal lines show the medians of ρ for individual subjects. The cross-hatched region shows where values of ρ are statistically insignificant. Open circles and ×s are for special sessions, see text.)

Figure 2 shows that four of those five were just marginally insignificant.

Without seeing the actual pictures used, it is hard to grasp the range of stimuli handled by the subjects in the experiment. Figures 3–5 contain representative stimuli for each experiment in black and white instead of the original color. The pictures were chosen from among those that no more than one pigeon misclassified, and in many cases no pigeon misclassified, on first viewing. The pictures in Figures 3–5 were chosen to suggest the variety of stimuli, although four pictures per experiment barely

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[1] The small daily variation in the numbers of positive and negative stimuli causes a small variation in the region of statistical insignificance. We have omitted this complication in Figure 2 because it would have added nothing to the data.

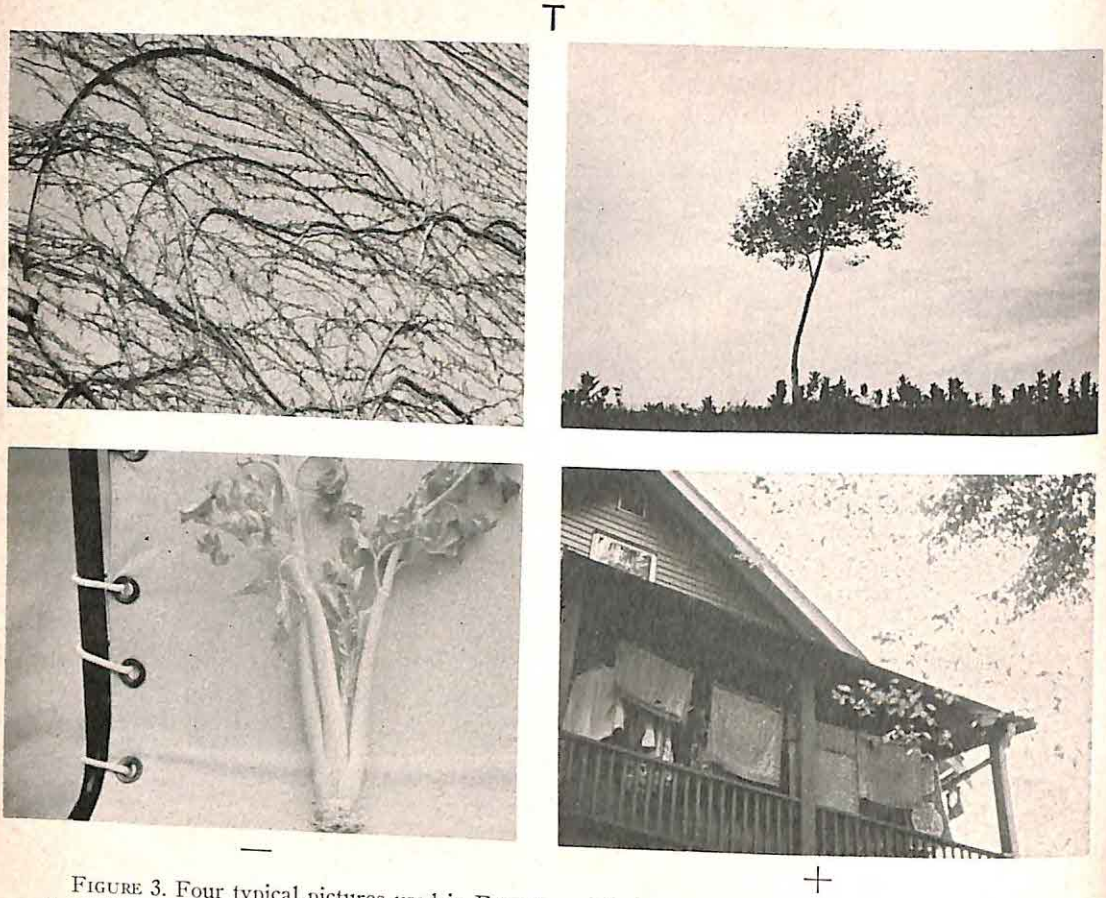

FIGURE 3. Four typical pictures used in Experiment T (trees), which were correctly classified by at least three of the four pigeons. (Negative stimuli are on the left; positive, on the right. The upper left picture shows a vine climbing on a cement wall; the lower left, celery.)

begin to do justice to the hundreds of pictures actually used.

In summary, from aggregate measures of performance and from an inspection of the pictures themselves, it is clear that the pigeons used principles of classification that approximate those we use ourselves, at least in complexity.

Concordance

Statistically, the problem of concordance resembles the problem of agreement among a group of judges at, for example, a flower show. Each judge ranks all the entries independently of the other judges. For any pair of judges, agreement may be evaluated with a measure such as Spearman's rank correlation. For more than two judges, a convenient measure is Kendall's coefficient of concordance, W (Hays, 1963; Kendall, 1948; Siegel, 1956), which is closely related to the average of the Spearman rank correlations between all pairs of judges. Unlike correlation coefficients, W falls between 0 and 1.0, with complete concordance equal to 1.0. For sizable (> 7) numbers of items to be ranked (n) the statistical significance of W is approximated by the chi-square distribution with $n - 1$ degrees of freedom.

For each session of new pictures, two coefficients of concordance were calculated, one for the ranked rates of responding to positive stimuli and one for that to negative stimuli. We were thus assessing the *degree* to which the pigeons agree, above and beyond their agreement about which pictures were positive or negative. Had W

been calculated for the entire day's session, then it would have reflected concordance owing to the discrimination between positive and negative stimuli. Any concordance in our analysis, however, is not explained by the separation of the ranks of positive and negative stimuli.

Table 1 gives the values of W for the 10 test days per experiment, where possible. Owing to several minor imperfections in the record of individual subjects, 3 days of the 30 are missing. The probability level is generally a function of W, but not exactly, for the chi-square corresponding to each value of W depends on both the number of pictures and the number of subjects, both of which varied to some extent. Nevertheless, a clear pattern emerges. Intersubject agreement is consistently and markedly greater for negative stimuli than for positive. Assuming no artifacts, concordance can only be shown when the pictures in a group vary in discriminability for the subjects individually. That is to say, each subject must find at least some pictures harder to classify than others. Then, concordance shows up when the subjects agree to some extent about which are hard and which are easy (or not so hard). If each subject finds all the pictures equally hard or equally easy, then concordance must be absent, for concordance is, like any correlation coefficient, a measure of covariance. The greater concordance for negative pictures may, then, be as much a reflection of greater negative picture variance as of anything else.

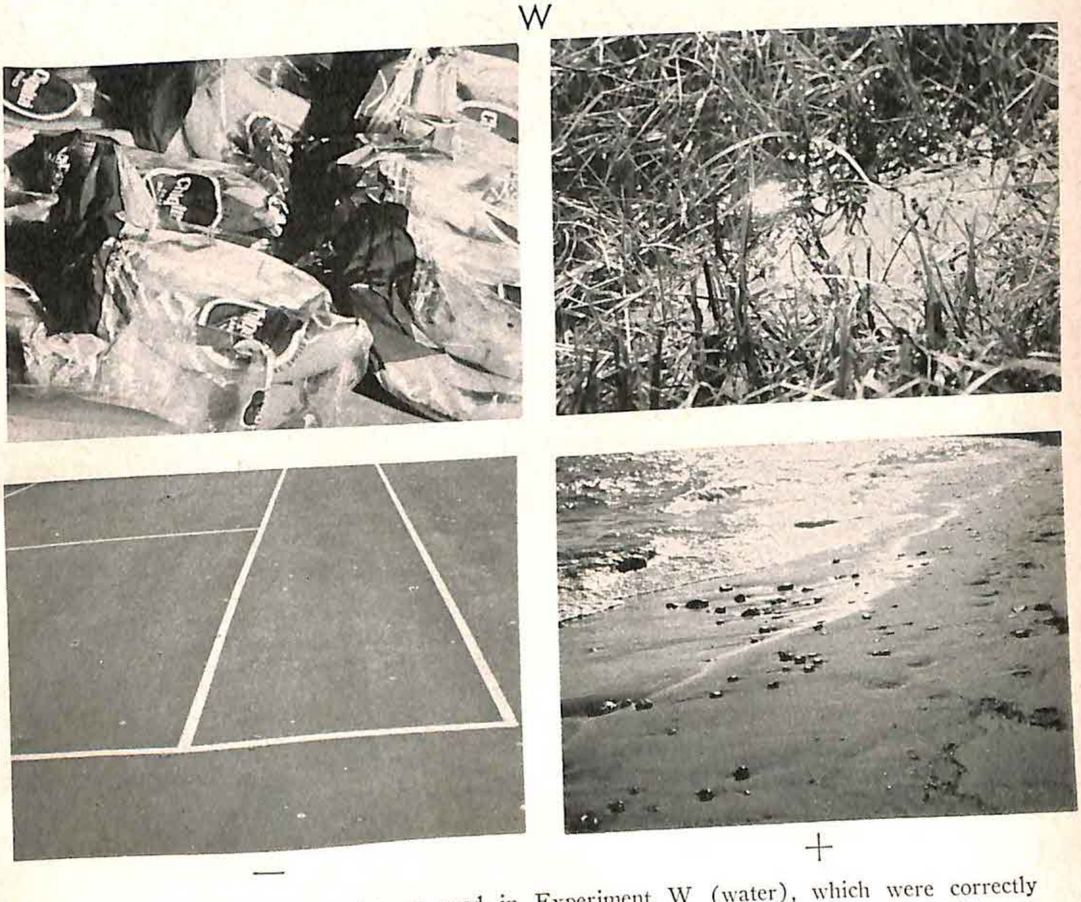

FIGURE 4. Four typical pictures used in Experiment W (water), which were correctly classified by at least two of the three pigeons. (Negative stimuli are on the left; positive on the right. The upper left picture shows cellophane bags of bananas; the upper right shows a small puddle through vegetation.)

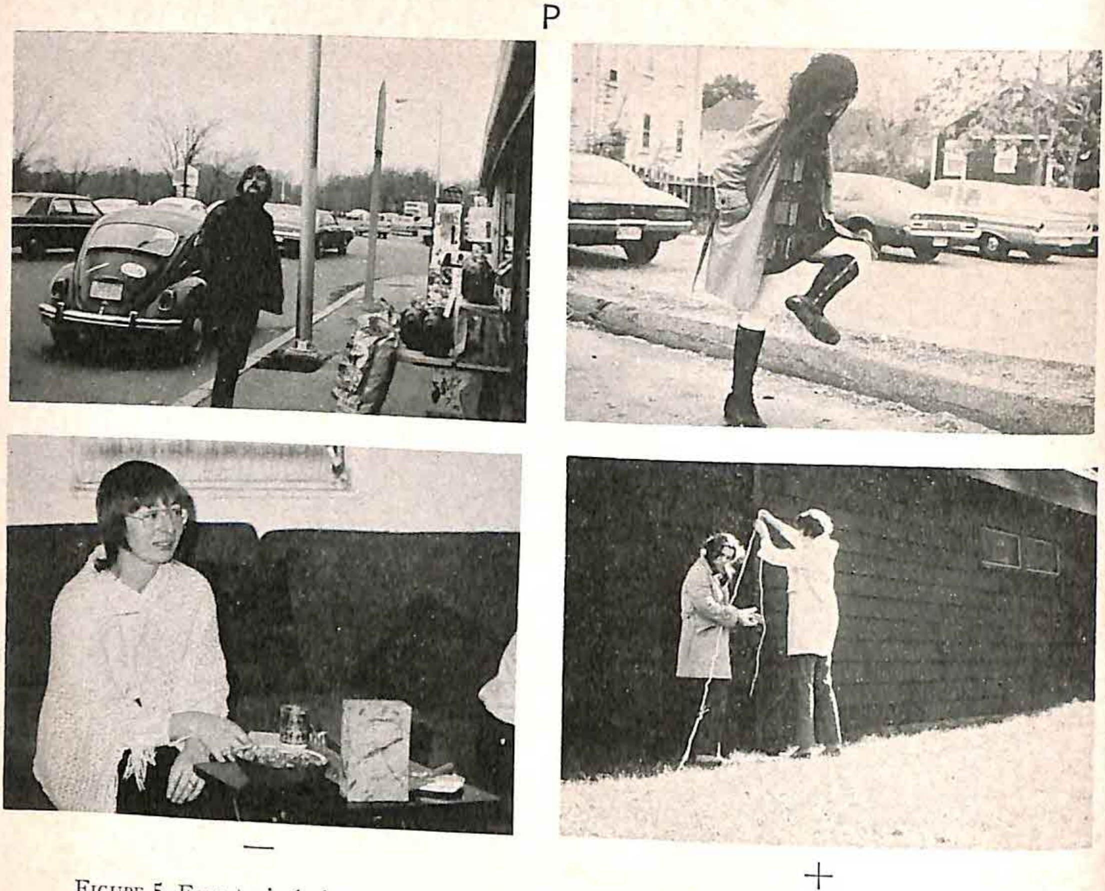

FIGURE 5. Four typical pictures used in Experiment P (specific person), which were correctly classified by at least three of the four pigeons. (Negative stimuli are on the left; positive, on the right. The upper left picture shows the subject's husband wearing her scarf; the lower left shows a different woman in the subject's apartment.)

This hypothesis can be tested by exploiting a feature of our procedure. Consider a session containing 40 positive and 40 negative stimuli. Perfect discrimination would be when all the rates to the positive ranked above all the rates to the negative stimuli. An error consists of ranking a positive stimulus among the negative stimuli, someplace among the bottom 40 ranks. However, this error will necessarily move one negative stimulus rate up among the top 40 ranks (assuming some negative-stimulus rates above 0). There is, in short, a corresponding negative misclassification for every positive misclassification and vice versa, a "true" error and, as a by-product, a "displacement" error. Greater negative-stimulus variance reduces to the hypothesis that true errors are more likely to be to negative stimuli than to positive, which is to say, to be false alarms rather than false dismissals.

The distributions of ranks of errors confirm that negative stimuli produced the greater number of true errors. Displacement errors should crowd the midrank, since they are being pushed down (for positive stimuli) or up (for negative stimuli) by the intrusions into the correct half of the ranks. In contrast, true errors may be distributed in any pattern within the incorrect half of ranks, depending on the degree of certainty in the erroneous ranking. Figure 6 presents the relevant analysis. Data were pooled across all subjects in each experiment and across the 10 sessions used for the coeffi-

TABLE 1

COEFFICIENTS OF CONCORDANCE, W

Session	Trees[a]		Water[b]		Person[a]	
	Positive	Negative	Positive	Negative	Positive	Negative
1			.49**	.54***	.41***	.55****
2	.27*	.47****	.40*	.66****	.38**	.40***
3	.36**	.46****	.43*	.63****	.36**	.44****
4	.22*	.47****	.53**	.69****	.52****	.54****
5	.28*	.31*	.41*	.52**	.39**	.48****
6	.55****	.55****	.41*	.55***	.27*	.48****
7	.25*	.39**	.60****	.55***	.27*	.59****
8	.38**	.46****	.38*	.63****	.21*	.63****
9	.57****	.33**	.46**	.63****		
10			.33*	.54***	.32*	.54****

[a] $n = 4$.
[b] $n = 3$.
* $p > .10$.
** $.01 < p \leq .10$.
*** $.005 < p \leq .01$.
**** $p \leq .005$.

cients of concordance in Table 1. The abscissa gives the incorrect half of ranks ("worst 40") in blocks of 10. For the positive stimuli, this is the 40 lowest ranks of rate of responding; for the negative stimuli, it is the highest 40. To make them directly comparable, the abscissa goes from the midrank to the end for both functions. That is to say, the block called 50 comprises the 41st–50th rank for positive and negative stimuli, but the ranking went from high to low rates for the positive stimuli and from low to high for the negative, and so on.

The ordinate shows the proportion of negative or the proportion of positive stimuli contained in the corresponding block. For example, in Experiment T, .112 of all positive stimuli were between the 41st and 50th rank, but only .090 of all negative stimuli were. The block called 80 shows what proportion of all positive stimuli were in the lowest 10 rates of responding and what proportion of all negative stimuli were in the highest 10 rates of responding. For Experiment T, the figures were .023 and .039, respectively.

With equal numbers of positive and negative stimuli, the four values plotted for the two curves for each experiment in Figure 6 would have summed to equality.[2] (Figure 6 shows in decimals what the actual total proportions were, reflecting the slight excess of negative stimuli.) In each experiment, the curve for positive stimuli is more sharply decreasing than that for negative stimuli, thereby showing more crowding below the midrank. This indicates that there were more displacement errors for positive stimuli than for negative or, inversely, that there were more true errors for negative stimuli than for positive. We may therefore infer that the subjects were more prone toward false alarms than false dismissals. Whatever its origin, the asymmetry may account for the greater concordance among negative stimuli.

A control session eliminated one possible source of the greater negative-stimulus variance in ranks, based on our method for correcting rates of responding. For positive stimuli, we used response rate prior to the first reinforcement; for negative stimuli, response rate prior to the penalty for responding (see section above entitled Discrimination). Consequently, the average durations for positive and negative samples differed, although the distributions overlapped. The difference between positive and negative

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[2] When there are 40 positive and 40 negative stimuli, the proportion of correct and incorrect instances can be represented as: $P_C + P_I = 1.0$, and $N_C + N_I = 1.0$. The errors comprise P_I, the proportion of positive stimuli ranked below the midline, and N_I, the negative proportion above the midline. With equal numbers of positive and negative stimuli, the proportions in the upper 40 ranks (for the 40 highest rates) must be: $P_C + N_I = 1.0$. By substitution, it follows that $P_I = N_I$.

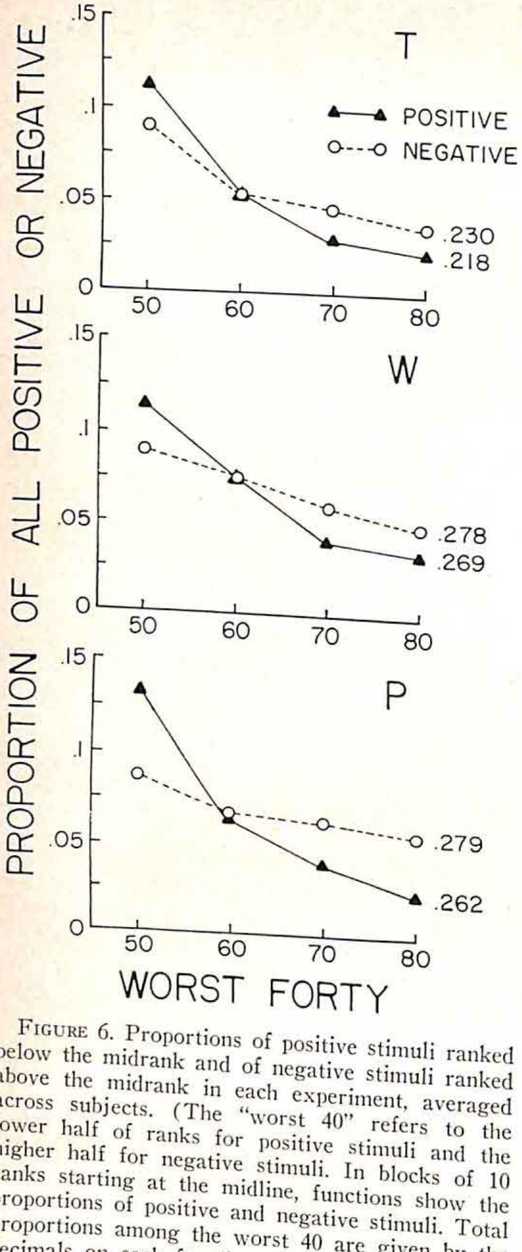

FIGURE 6. Proportions of positive stimuli ranked below the midrank and of negative stimuli ranked above the midrank in each experiment, averaged across subjects. (The "worst 40" refers to the lower half of ranks for positive stimuli and the higher half for negative stimuli. In blocks of 10 ranks starting at the midline, functions show the proportions of positive and negative stimuli. Total proportions among the worst 40 are given by the decimals on each function. Positive-stimulus functions include the false dismissals; negative-stimulus functions, the false alarms.)

samples ranged around 7 to 12 sec; in proportional terms, the negative samples were anywhere from 20% to 50% longer. It is possible that the longer negative stimulus durations allowed more concordance to develop, which might have accounted for the differences in Table 1 and Figure 6.

To check this possibility, the seven subjects in Experiments T and W were run with an additional session of new pictures and a modified definition of the corrected rate for negative stimuli. The variable-interval schedule for food reinforcement was run in the computer during both positive and negative stimuli. During positive stimuli, it continued to program reinforcements as before. During negative stimuli, it timed the interval over which rate of responding was calculated, so that the peck that would have been reinforced had the stimulus been positive shuts off the response-rate recorder. The average durations for the corrected rates to positive and negative stimuli for the seven subjects on the additional test day had a mean difference of .7 sec, with positive durations longer.[3]

The question is whether concordance still favors negative stimuli, even when the difference in durations is eliminated. For Experiment T, the negative stimulus concordance was .44 and the positive stimulus concordance was .32. Using the chi-square approximation, the negative stimulus value was significant beyond the .005 level, and the positive stimulus value was insignificant at the .10 level. For Experiment W, the negative stimulus concordance was .52 and the positive stimulus concordance was .42. This positive stimulus value is again insignificant at the .10 level, and the negative stimulus value has an associated probability level between .10 and .01. From this, we can conclude that negative stimulus concordance is higher than positive even when the response-rate artifact has been eliminated.

A second source of the findings in Table 1 and Figure 6 has not been definitively excluded. All subjects in each experiment saw stimuli in the same order. Concordance may therefore reflect systematic variations in pecking during each session, which could differ for positive and negative stimuli. A sampling of product-moment correlations between rank and order-of-presentation number are shown in Table 2, in which the session numbers correspond to those in Table 1. Briefly, although most of the correlations are small, there is clear evidence for a trend toward lower ranks later in the session (shown by negative correlations), such as may be produced by food satiation, for example. Even though these correlations doubtless contribute to the concordance among subjects, it does not seem possible to specify the size of the contribution or to relate it to the difference between positive and negative stimuli given the present results.

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[3] This modified definition of corrected rate was used for all sessions and all subjects in the special sample, to be discussed, which demonstrates that discrimination itself was not dependent on the definition of corrected rate. In general, we found no evidence that this change made any difference. Indeed, from casual inspection it seems that much the same picture would have emerged had we not corrected rates at all, but we have not redone our entire analysis with uncorrected rates.

For purposes of examining correlated errors, we identified the eight worst positive and eight worst negative stimuli in each of the 10 sessions of new stimuli used in this analysis. That is to say, we retrieved for each subject in each session the eight positive stimuli that obtained the lowest ranks in rate of responding and the eight negative stimuli that obtained the highest ranks. Given the contingencies of the experiments, these stimuli must be taken as worst in the sense that they come closest to misclassification; hence, they are called the "worst cases" in the ensuing analysis.

The pictures shown earlier in Figures 3–5 were considered correctly classified because they fell into these eight worst cases for no more than one subject in each experiment. Here we focus on those that fell among the worst cases for several subjects. Figures 7–9 show two positive and two negative pictures for each experiment that at least $n - 1$ pigeons misclassified, again in black and white instead of color. The pictures in Figures 7–9 are mostly, though not invariably, relatively difficult for human observers. Even if errors were uncorrelated, however, some pictures would fall among the worst cases for $n - 1$ subjects by chance. The preponderance of difficult stimuli in Figures 7–9 is consistent with the conclusion that the subjects' rule for classification approximated the experimenters'.

TABLE 2

Correlations Between Rank of Responding and Order of Presentation

Subject	Session 2 +	Session 2 −	Session 8 +	Session 8 −
		Trees		
51H	.04	−.22	−.15	−.35
56H	.43	−.19	−.05	−.20
91C	−.26	.06	−.34	−.23
244C	−.27	−.16	−.15	−.45
		Water		
24C	.09	.23	.47	.02
63H	−.42	.04	−.61	−.45
92C	.35	.10	−.09	−.35
		Person		
7C	−.12	−.29	−.34	−.37
8C	.10	−.38	−.08	−.37
44C	−.32	−.24	−.30	−.06
45C	−.07	−.36	−.06	−.31

Special Samples

The data so far summarized do not prove that the subjects' categories were isomorphic with the experimenters', though they suggest it strongly. It remains possible that an incidental flicker of light, a stray sound of switching circuitry, or something comparable was correlated with positive or negative stimuli and controlled performance without our knowledge. Although patent artifacts were deliberately guarded against, more subtle ones may not have been anticipated. As a control for stimulus artifacts, two sets of 80 pictures for each experiment were handpicked. One set of pictures was picked to be easy and the other hard by the experimenters' own standard of judgment. The range of difference was roughly comparable to the easy and hard stimuli in Figures 3–5, but the pictures actually used were from among those never previously shown to the pigeons. Then, on 2 consecutive days, each pigeon saw first one, then the other special samples. Some pigeons saw the "hard" pictures the first day and the "easy" pictures the next; other pigeons had the reverse order. The procedure was the same as for the 10 test sessions described earlier, except for the calculation of corrected rates (see

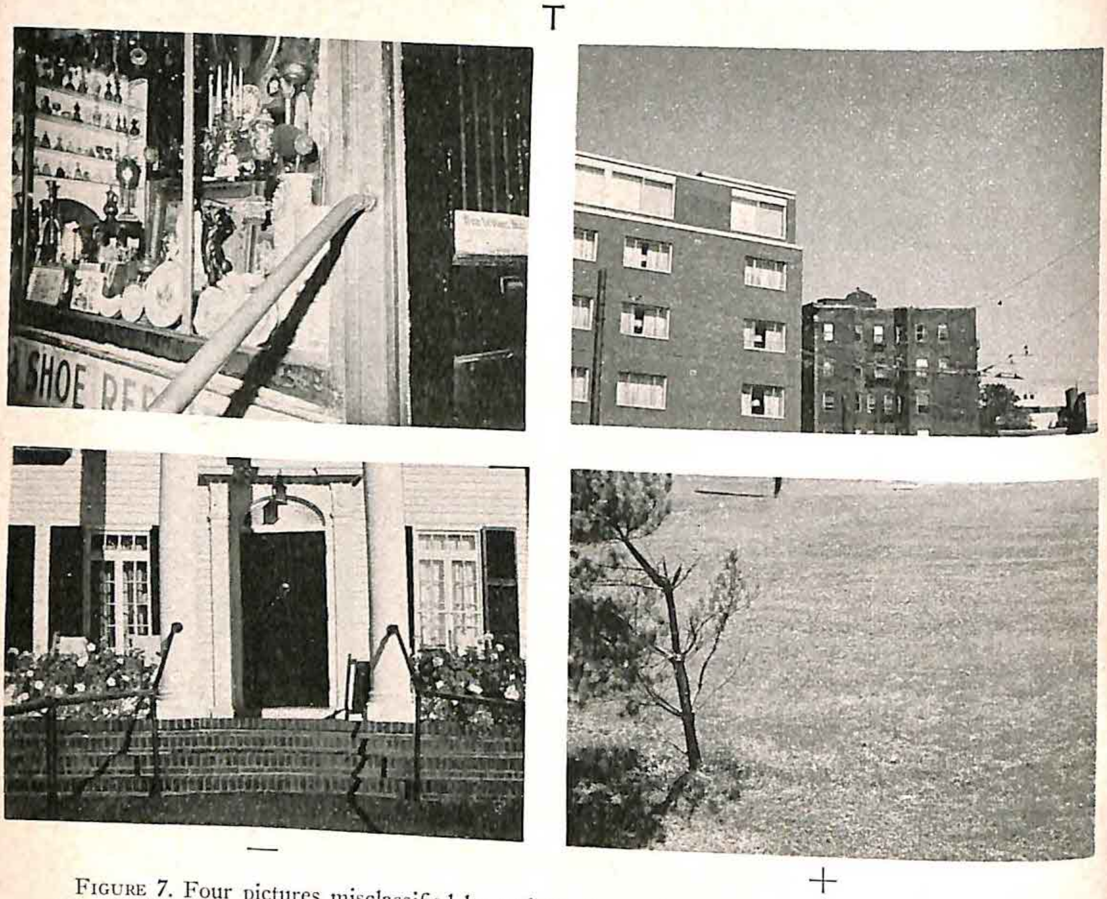

FIGURE 7. Four pictures misclassified by at least three of the four pigeons in Experiment T. (Negative stimuli are on the left; positive, on the right. In the upper right picture, trees are just barely visible.)

Footnote 3). Table 3 presents the main results of this special procedure in terms of ρ. The values of ρ also appear in Figure 2 as crosses (hard pictures) and open circles (easy pictures), in the order in which they were obtained.

If the pigeons' stimulus classes were essentially isomorphic with ours, and if ρ can be accepted as a valid measure of discrimination, then the expected pattern would be that shown by Subjects 56H, 91C, 63H, and 8C, for whom the easy pictures produced relatively high values of ρ while the hard pictures produced low ones. The five other pigeons deviated from the ideal pattern. Subjects 51H, 92C, and 244C found the hard pictures hard, but the easy pictures were no easier than the unselected pictures in the earlier tests. Subjects 24C and 44C, who found both hard and easy pictures hard, had seen the hard pictures first. The poor performance with the easy pictures may show the aftereffects of the hard pictures. Finally, Subjects 7C and 45C found the hard pictures only slightly harder than the easy ones, with both sets well within the range observed with unselected pictures.

As a whole, hard pictures had the more consistent effects. For 9 of the 11 subjects, discrimination fell to virtual statistical insignificance. For the other 2 subjects, discrimination was below the earlier median level but not markedly. Easy pictures produced a more complex mixture of effects. Five subjects discriminated them about as well as the unselected pictures. For 4 subjects, discrimination was what might be called supernormal. And discrimination was

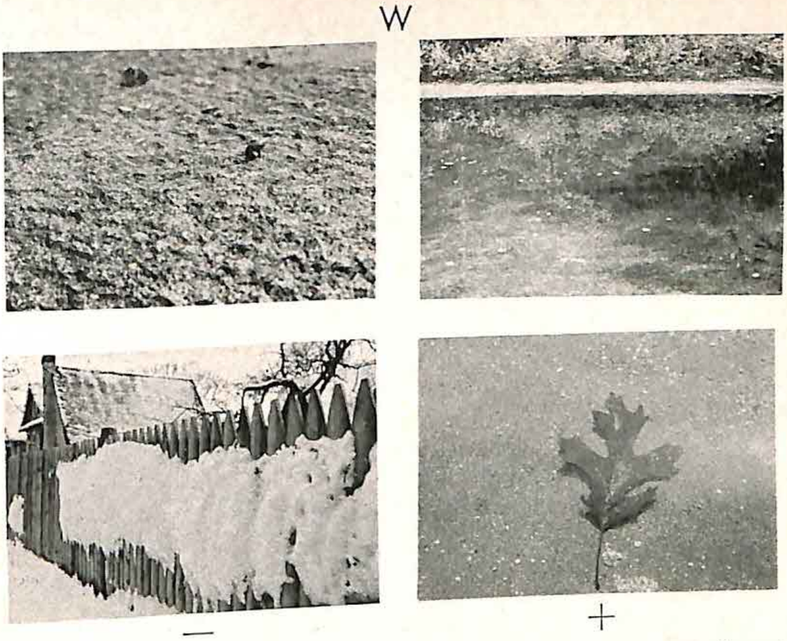

FIGURE 8. Four pictures misclassified by at least two of the three pigeons in Experiment W. (The upper right picture shows a swampy field in front of a road; the lower right picture shows a leaf under water on a sandy bottom near the shoreline.)

absent for 2 subjects that saw the easy pictures after having been disrupted by the hard ones. The overall effects of the hard and easy samples should dispel any doubt that the controlling stimuli were visual for all the subjects except possibly 7C, which showed no effects of either hard or easy stimuli. Moreover, it seems that the visual classes controlling performance were at least correlated with what we ourselves are looking at, though perhaps imperfectly. The test with special selections uncovered differences among the subjects that were not evident in their performances with hundreds of unselected pictures.

DISCUSSION

The ability to discriminate open-ended classes of stimuli poses problems at two levels of analysis. First is the analysis of the features enabling a subject to tell whether an object is a member of a class—whether a picture contains a tree, for example. Second is the analysis of the properties of classes that render them discriminable. Let us assume that the thousands of pictures used in the present experiment could have been divided into some sets that pigeons could, and some that they could not, learn to sort. Above and beyond the question of how they sort within any given classification problem is the question of distinguishing between solved and unsolved problems.

The traditional explanation at the first level of analysis is a theory based on common elements, recently exemplified by Blough (1975) and Rescorla (1976). Trees, according to this theory, have something specific in common, for example, a certain shape or texture or color or combination of them. And if common elements suffice for the first level, they also solve the problem of the second level. A classification would presumably be discriminable only if it were based on discriminable common elements. However, having looked at the hundreds of instances used here or even at the two positive pictures in Figure 3 (let alone the tens of thousands involved in real-life discriminations), we cannot begin to draw up a list of

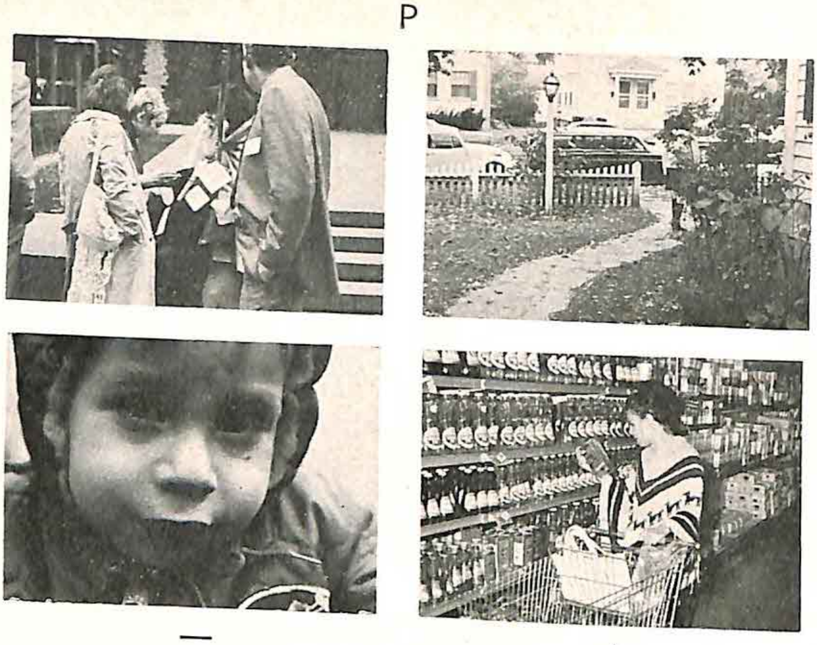

FIGURE 9. Four pictures misclassified by at least three of the four pigeons in Experiment P. (The lower left picture shows the subject's child, who occasionally appears with her in positive stimuli.)

common elements. To recognize a tree, the pigeons did not require that it be green, leafy, vertical, woody, branching, and so on (overlooking the problem of common elements nested within terms like leafy, vertical, woody, and so on). Moreover, to be recognizable as a nontree, a picture did not have to omit greenness, woodiness, branchiness, verticality, and so on. Neither could we identify common elements in the other two experiments.

TABLE 3

DISCRIMINATION INDEX, ρ, WITH SPECIAL SAMPLES OF STIMULI

Subject	Easy	Hard
	Trees	
51H	*.814*	.554
56H	.980	*.573*
91C	*.963*	.551
244C	.742	*.523*
	Water	
24C	*.574*	.578
63H	.881	*.590*
92C	.744	*.616*
	Person	
7C	.799	*.748*
8C	.886	*.514*
44C	*.497*	.450
45C	*.883*	.808

Note. Italicized indexes show the second of the two sessions of tests with the special samples.

If not common elements, what? No other theory is so easily characterized, though in crude terms an alternative suggests itself. Pigeons respond to clusters of features more or less isomorphic with the clusters we respond to ourselves. The green should be on the leaves, if either green or leaves are present. However, neither is necessary or sufficient. The vertical or branching parts should be the woody parts, although neither of these features is necessary or sufficient either. What we see as trees comprises a complex list of probabilistic conjunctions and disjunctions, the discovery of which would require far more effort than seems justified by any possible benefit. Insofar as no visual element or configuration of them is either necessary or sufficient, there can be no single prototype or schema defined at the level of visual arrays, a conclusion much like Wittgenstein's notion of "family resemblance" (Wittgenstein, 1953).

After a few dozen daily sessions amounting to no more than about 700 different instances of trees and nontrees, the pigeons readily sorted another 800 instances, none of which was identical to each other or to the original 700. New pictures were often discriminated with higher accuracy than pictures already seen. Even more impressive generalization was shown by pigeons in a study using pictures of people as the positives class (Malott & Siddall, 1972), in which just one or two dozen pictures were used in the original training. From this scanty exposure, the subject generalized to new instances without apparent limit. The minimal case appears to be Cerella's (Note 1) experiment, in which pigeons may have generalized to the class of silhouettes of oak leaves from having been trained with 1 positive instance and 40 negative instances.

These experiments could be considered cases of stimulus generalization. But unlike the typical experiment using standard physical dimensions as the stimulus variable, we know of no relevant physical variable. Instead of wavelength or decibels or seconds, the dimensions here can, at present, be characterized only in the language of objects, not stimulus attributes, a distinction embodied in Konorski's (1967) concept of the "gnostic field." The pigeons display semantic generalization in the sense that we can describe their behavior better by noting what the pictures are pictures *of*, rather than by what the pictures themselves *are*. Having seen a collection of patterns that we recognize as pictures of trees, the pigeons generalize to novel patterns that we also recognize as pictures of trees. The constancy is in the world of distal objects, not in that of proximal patterns.

Little is resolved, however, by saying that the performance is a form of generalization, semantic or otherwise, for we have no satisfactory theory of generalization either. The semantic categories used by the pigeons must have physical specifications, for the pictures are, in fact, nothing but optical arrays. They apparently see a stalk of celery, leaves and all, as a nontree, although it is green, leafy, etc. On the standard optical continua, the celery is clearly close to many of the trees one sees, but the pigeons shift to some other system of classification when it pays to do so, as it does here. Data from traditional generalization experiments show that pigeons are sometimes able to operate along the standard visual attributes such as color, shape, orientation, etc., about as well as we do, but, like us, they have the capacity to deal with object categories too.

The capacity doubtless has something to do with evolution. To the question of what distinguishes discriminable stimulus classes from the indiscriminable, a common response brings in the creature's germ plasm. It is held that organisms are disposed to group together those stimuli that signify objects with similar psychological consequences. For example, trees, bodies of water, and people have long been both important and common in the pigeon's natural environment. By now, these objects may have had enough evolutionary significance to be somehow represented in the genes. The evolutionary account is used not only by psychologists confronted by the data on categorization and generalization but also by philosophers (e.g., Quine, 1969) discussing the deficiencies of traditional empiricism.

Assuming for the sake of argument that there are genetic constraints on the categories that creatures induce, it remains to be shown how those constraints operate functionally. For example, what is it about the stimuli in Experiment T that triggered the genetic predisposition to induce trees but in Experiment W to induce water? The triggering factor could not have been greenness, woodiness, and so forth—those elements whose inadequacy led us to the genetic hypothesis in the first place. The genetic hypothesis shifts the locus of the problem of classification but does not solve it.

Although we did not discover the optical details of the categories used by our pigeons, certain conclusions can be drawn. The present findings show that classes as complex as trees can be defined by static features. Successful discrimination of still photographs proves that a pigeon need only look at examples of a class in order to activate its rules. Since, in nature, interactions are with objects as a whole, it is not a foregone conclusion that two-dimensional projections could work. It is also not a foregone conclusion that movement could be omitted. It has been suggested that people unaccustomed to looking at pictures have trouble finding objects (Deregowski, 1972), but the subjects in such studies get less practice than did the pigeons in ours.

It is also not a foregone conclusion that pigeons and people generalize similarly, yet they clearly did to a degree. For some pigeons, our "easy" instances proved easy and our "hard" instances hard. The pigeons were housed for several years in a room with windows on the seventh floor of a building in a residential neighborhood. From the pigeons' home loft, trees are visible in the distance (> about 200 yards; 180 m); the only visible water is in the drinking cups and perhaps in the occasional mop pail. The pigeons never saw the photographic subject in Experiment P in person. Nevertheless, pigeons and people converge on similar, if not equivalent, categories. The results suggest that the pictures used as stimuli activate a category rather than define it and, second, that the activated category draws on something other than past experience.

This is not to postulate innate categories for pictures of trees, water, and persons. Although that may seem a possibility for Experiment T, the analogous conclusion for Experiment P is manifest nonsense—an innate category for recognizing a particular young woman living in Cambridge around 1970. Instead, the innate ingredient in these discriminations must operate less specifically. Given a finite set of varying stimuli, the pigeon activates a particular category out of the limitless number of categories more or less equally well determined by the same set. It is in this narrowing of the range of possible categories that innateness seems to be expressed.

Data on human concept formation have indicated that a set of varying stimuli may be remembered as the central tendency of the variations called the "prototype" or "schema" (Posner & Keele, 1968). Even if the person never sees the prototype itself, retention will be more enduring for the prototype than for any of the individual exemplars whose central tendency defines the prototype (Posner & Keele, 1970). We noted earlier the trouble with a prototype defined at the level of the stimuli. In addition, there is some question about the generality of the finding for human subjects.

Rosch (1973, 1975) showed that the inferred prototypes deviate from the simple central tendency of exemplars when the stimulus domains are more "natural." Thus, whereas Posner and Keele used random configurations of dots as the stimuli in their experiment, Rosch used colors and forms in hers. The colors and forms that were prototypic tended to be perceptually unitary shades of red, green, and so on, or simple geometrical figures like squares, even when the exemplars were biased away from these prototypes and even when the subjects were drawn from populations (e.g., young children or New Guineans) that have not learned words for them. For Rosch, the "central tendency" characterization of prototype is augmented into a "best" or "clear-case" characterization. Pure, deep red becomes a prototype not because it is at the central tendency but because it meets a certain requirement or set of requirements so well. But this raises yet another question, as to the source of these requirements. In her answer, Rosch contrasts "perceptually given" prototypes, such as red or square, with "semantic category" prototypes, such as *bird* or *chair*. For the former, the best case prototype is "physiological," which means innate. For the latter, it is cultural, hence empiricistic, according to Rosch. The best cases of *chair, bird,* and so on, develop through some sort of learning process, which includes at least a component of taking a central tendency, though perhaps with nonlinear weighting of attributes. A quite similar notion, couched in neurophysiological vocabularly, is Konorski's (1967) gnostic unit, a neural element that somehow registers a percept of a distal object rather than of the proximal stimulus.

Rosch distinguishes between prototypes based on innate constraints and those based on experiential averaging—for example, *red* on the one hand, and *bird* on the other. *Trees, bodies of water,* and *person,* being semantic, would be experiential in her theory. But it seems improbable, to say the least, that the same experience-averaging procedure—however complex—could be applied to our samples of positive and negative stimuli in each of the three experiments and come up with roughly the right (i.e., the human) prototype in each case. And it is still more implausible applied to the previously noted studies by Malott and Siddall (1972) and Cerella (Note 1). It is more plausible to conclude that pigeons tend innately to infer a tree category from instances of trees, a familiar-person category from varying instances, and so on, more or less as we do ourselves. The properties of the inferred class arise from the joint constraints imposed by the stimuli and by innate factors. If two organisms form equivalent classes from the same stimuli, it is because they share not only the stimuli but also the genetic constraints.

Our data support at least a partial isomorphy in the inferred classes between pigeons and people. The pigeons discriminated reasonably, and they were in some cases suitably helped or hindered by the special easy or hard samples. These results favor the hypothesis of isomorphy. On the other hand, discrimination was not perfect and the special samples were not invariably effective, especially the easy ones. These results may point to something less than total isomorphy between the categories of the people and the pigeons involved here. However, we cannot say whether the difference is experiential or innate, for it could be either or both.

The most surprising result here may be not that pigeons can use open-ended natural categories but that the three experiments produced data as similar as they did. The sheer levels of discrimination (Figure 2), the concordance among subjects (Table 1), and even the pattern of true errors and displacement errors (Figure 6) are almost duplicated in each experiment. If we had imposed rigid criteria in the selection of pictures, such invariances would perhaps be less unexpected. But in fact, we had no such criteria. Each experiment produced more false alarms than false dismissals, more concordance for negative instances, and most discriminations in the range of $\rho = .7–.9$. The first two of those findings are consistent with the hypothesis of a smaller dispersion of positive stimuli than negative, a plausible finding given the stimulus materials. However, at this point, it must remain a hypothesis, not a conclusion, for the stimulus metric is itself unknown. Positive and negative stimuli may also differ inasmuch as correct positive-stimulus behavior produced food, whereas correct negative-stimulus behavior avoided delay. Further study is needed to see how much our method itself constrained both the levels of discrimination and the patterns of errors and concordance.

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Duration Discrimination by Rats

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A psychophysical procedure was used to determine the difference limen for the duration of a signal that ranged from .5 to 8.0 sec. The accuracy of three rats in keeping track of the duration was assumed to be limited by three factors: (a) inattention to the signal on some trials, (b) variability in starting to time the duration when the signal begins (and/or in stopping to time the duration when the signal ends), and (c) factors related to signal duration itself. A generalized Weber model provided a better approximation to the growth in the difference limen as a function of signal than a generalized Counter model.

In many studies of classical conditioning and instrumental training, the performance of an animal is related to the expected time to the next reinforcing (or punishing) event. For example, if reinforcement is made available at fixed intervals (FI schedule), an animal responds increasingly rapidly as the time of the next reinforcement approaches (e.g., Dews, 1970); if reinforcement is made available only if the animal withholds responding longer than a criterion time (DRL schedule), the distribution of interresponse times is closely related to the minimum reinforced interresponse time (e.g., Kramer & Rilling, 1970); if a response delays a shock for a fixed time (Sidman avoidance schedule), the avoidance response rate increases as the time of the next aversive event approaches (e.g., Gibbon, 1972); when shocks occur at fixed intervals of time (temporal conditioning), the rate of a positive reinforced response decreases as the time to the next shock approaches (e.g., LaBarbera & Church, 1974); when food is delivered at a fixed time after signal onset (inhibition of delay), the response rate increases as the time to next reinforcement approaches (e.g., Pavlov, 1927). Examples of this sort are numerous, and they suggest that animals are capable of estimating the time of occurrence of an event and adjusting their performance in an appropriate manner.

Psychophysical procedures provide the simplest method for studying time discrimination of animals. In the psychophysical situations we will consider, the subject is given two alternatives, equivalent to answering "short" and "long." The experimenter presents a signal; the subject classifies its duration and responds "short" or "long" with respect to some criterion.

Two psychophysical studies of time discrimination of pigeons have been reported. Kinchla (1970) trained pigeons to discriminate a 5-sec tone from tones of shorter duration (2, 3, and 4 sec). At the most difficult discrimination, the pigeons were correct on about 60% of the trials, and the performance was better on the easier discriminations.

The most thorough psychophysical study of time duration in animals was performed by Stubbs (1968). He trained pigeons to make one response when a short-duration stimulus occurred and a different response when a long-duration stimulus occurred. In the first experiment stimuli varied from 1 through 10 sec, and the criterion was 5 sec. A standard psychometric function (roughly ogival in form) was obtained when the percentage of long judgments were plotted as a

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function of the logarithm of the stimulus duration. Similar psychometric functions were obtained for other ranges (2 through 20 sec, and 4 through 40 sec). In fact, when plotted against the proportion of the longest interval in the range, the functions were indistinguishable. This means that the difference limen was equal to kT, where T is the duration of the standard and k is a constant. This result provides strong support for the applicability of Weber's law to animal time discrimination: The increment in duration required to produce any particular level of discriminability is a constant proportion of the standard duration.

In a study of discrimination of durations between 0 and 16 sec, Tarpy (1969) found additional support for Weber's law. Rats were trained to press a lever to escape from electric shock. One lever turned off the shock after a standard delay of T sec; the other lever turned off the shock after T' sec. Twenty-eight different groups were tested, each with one value of T and T'. The conclusion was that the difference between T and T' necessary to produce 75% preference for the shorter duration was a constant proportion of the standard duration.

Weber's law has not fared so well in studies of human duration discrimination. Several parametric studies (Blakely, 1933; Getty, 1975; Scott, 1935) have reported the Weber fraction to be a broad U-shaped function of duration. These studies suggest that Weber's law is approximately correct only for durations between .5 and 2.0 sec. There now appears to be a discrepancy. Weber's law is rejected for human discrimination of durations longer than about 2 sec but accepted for animal discrimination of durations much longer than 2 sec. Because there is remarkably little data on psychophysical duration discrimination by animals, further research may determine whether or not it is possible to have a single underlying theory of duration discrimination for both animals and people.

Theory of Duration Discrimination

There are three limitations to an animal's accuracy in estimating temporal duration: (a) factors present on some trials but not on others, for example, inattention to the task; (b) factors unrelated to signal duration that are present on all trials, for example, signal onset and termination; and (c) factors related to signal duration that are present on all trials.

Factors present on some trials only. Although an animal may be trained to respond to a particular dimension of a stimulus, such as its temporal extent, it may fail to use this cue on some trials. This may be a result of distraction during the presentation of the signal, failure to recall the duration of the signal, failure to recall the instructions (i.e., which response is reinforced after each signal), or side preferences. For expository purposes, we refer to all of these factors as *inattention.* We let a be the percentage of trials on which the animal is responding with respect to the stimulus, P_a be the probability that the animal will be correct when it is paying attention to the stimulus, and we assume that an animal in a two-choice situation will be correct on 50% of the trials in which it is not paying attention to the stimulus. The observed probability (P_o) that the animal will be correct, then, is simply a weighted mean:

$$P_o = aP_a + (1 - a)\ .5. \qquad (1)$$

On an easy task we assume that the animal will always be correct if it is paying attention to the stimulus (i.e., $P_a = 1$). Even on an apparently easy task the animal may make some irreducible minimum number of errors (i.e., $P_o < 1$), and this error rate would not be further reduced by increasing the difference between the two stimuli. The error rate on such a task serves as an estimate of a. This correction for inattention has been used previously (Heinemann, Avin, Sullivan, & Chase, 1969). A more complicated expression would be required if animals were assumed to be partially inattentive during some trials, or if their inattention were related to signal duration.

Factors unrelated to signal duration that are present on all trials. Signal onset and termination is the prime example. In a review of psychophysical theories of human duration discrimination, Allan and Kristofferson (1974) emphasized an onset–offset model in which the variability in the subjective duration of a single is the result of (a) variability of the time from signal onset until the subject begins to internally time the duration, and (b) variability between signal termination and the time until the subject stops timing the duration. Since the signal onset and termination is common to all signal durations, this source of variability (V) is independent of the duration of a signal.

Factors related to signal duration. At the present time there is no general agreement about the way in which the variance of the subjective duration of a signal is related to the actual duration of the signal. One quantitative model, which we will call the Counter model, was developed by Creelman (1962). The basic assumptions are that pulses are emitted randomly in time and counted, and the subject's internal representation of time is equivalent to the number of pulses that have been emitted during a signal. Thus, according to this model the number of pulses during a signal of fixed length will be a Poisson-distributed random variable, and the growth of the variance of the internal representation of time ($\mathrm{Var}(T)$) will be directly proportional to the actual duration of the signal (T), where k_c is a constant:

$$\mathrm{Var}(T) = k_c T, \qquad (2)$$

or, equivalently, $SD(\mathrm{T}) = (k_c T)^{\frac{1}{2}}$.

An alternative model, which we will call the Weber model, assumes that the growth of the standard deviation of the internal representation of time ($SD(T)$) is a constant proportion of the duration of the signal (T):

$$SD(T) = k_w T, \qquad (3)$$

where k_w is the Weber constant, or equivalently, $\mathrm{Var}(T) = k_w{}^2 T^2$.

This is a major difference between the Counter model and the Weber model of duration discrimination. In the Counter model the standard deviation of subjective duration increases as the square root of time; in the Weber model the standard deviation of subjective duration is directly proportional to time.

Combination of factors related to signal onset, termination, and duration. The variance in the subjective duration of a signal is determined both by factors related to the duration of the signal and by factors unrelated to the duration of the signals (V). If these two types of factors are independent, they may be added. Thus, for the Counter model:

$$\mathrm{Var}(T) = k_c T + V_c, \qquad (4)$$

or, equivalently, $SD(T) = (k_c T + V_c)^{\frac{1}{2}}$. For the Weber model,

$$\mathrm{Var}(T) = k_w{}^2 T^2 + V_w, \qquad (5)$$

or equivalently, $SD(T) = (k_w{}^2 T^2 + V_w)^{\frac{1}{2}}$.

If the psychometric function were approximately a normal ogive, the difference limen (DL), which encompasses 25% of the underlying distribution, would be equal to .67 SD. Therefore, for the Counter model,

$$DL = .67(k_c T + V_c)^{\frac{1}{2}}, \qquad (6)$$

and, for the Weber model,

$$DL = .67(k_w{}^2 T^2 + V_w)^{\frac{1}{2}}. \qquad (7)$$

The linear form of the equation for both the generalized Counter model (Equation 6) and for the generalized Weber model (Equation 7) is:

$$DL^2 = k_1 T^p + k_2, \qquad (8)$$

where k_1, k_2, and p are constants. For the generalized Counter model, $p = 1$; for the generalized Weber model, $p = 2$. The central task for the present experiment was to identify the exponent that best describes the relation between the squared difference limen and the duration of the standard signal.

The psychophysical method we used with rats was extremely simple. On each trial an auditory signal lasted either for a standard duration or for a (longer) comparison duration. The left response was reinforced if the standard duration had been presented; the right response was reinforced if the comparison duration had been presented. The duration of the comparison on each trial was determined by an up–down method so that the rats were correct about 75% of the time. The major result of the experiment should be a function relating the difference limen to the duration of a standard signal.

METHOD

Subjects

The subjects were three male albino Norway rats (Charles River CD). They were experimentally naive and about 100 days old at the start of training. The animals were individually caged throughout the experiment, and each rat received ad lib water, and a daily 14-g ration of ground Purina chow mixed with about 25 cc water.

Apparatus

Four lever boxes (23.2 × 20.3 × 21.9 cm high) were used. The roof and the two side panels were made of transparent acrylic; the front and back walls were aluminum. The chamber floor was 16 parallel stainless-steel bars. Electric shocks were delivered to the grid through an autotransformer, isolation transformer, and 150-kΩ series resistance. Two retractable stainless-steel levers (1.3 × 5.1 cm) projected through the front panel, 5.1 cm above the floor. A pellet dispenser (Gerbrands Model D-1) delivered 45-mg Noyes precision food pellets through an opening in the front panel to a food cup. A 6-W houselight was attached to the exterior of the roof of each lever box. A Grason-Stadler noise generator (Model 901A) delivered a white-noise signal of aproximately 70 d*B* (A) into a speaker mounted behind the front wall of the lever box. Each lever box was housed in a large insulation board chamber designed to attenuate sounds and block visual stimuli. Each chamber was equipped with a blower and an acrylic window. Implementation of the procedure and collection of data were controlled by a time-shared PDP-12 computer.

Procedure

Pretraining. Each rat received one session of magazine training in which the levers were retracted, and one pellet was delivered each minute for 60 min. On the following session each animal was trained to press the levers. The left lever was inserted and 10 responses were reinforced; then only the right lever was inserted and 10 responses were reinforced. This alternation between the left and right levers continued until the rat had pressed each lever 30 times.

Training. During the next 55 sessions the rats were trained to press the left lever what a short (.5-sec) white-noise signal occurred, and the right lever when a long (8.0-sec) white noise occurred. On each trial either the .5-sec or 8.0-sec signal was presented. When the signal ended, both levers were inserted into the box. If the short stimulus had been presented, then a left-lever response was correct; if the long stimulus had been presented, then a right-lever response was correct. If the rat made the correct response, a pellet was delivered and both levers were retracted. If the rat made the incorrect response, both levers retracted from the box, a punishment was delivered (.5-sec shock of 45-V intensity), and the houselight was turned off for 10 sec. In either case, there was a 60-sec intertrial interval from a response to the next signal. A between-trials correction procedure was used as follows: If the response on the previous trial had been correct, the probability of either a long or short stimulus on the next trial was .5. If the response on the previous trial had been incorrect, the stimulus of the previous trial was repeated. The purpose of the correction procedure was to break up position habits. The first 15 sessions each consisted of 60 trials; the next 40 sessions each consisted of 100 trials.

Testing. On each trial the white-noise signal was presented for the standard time interval (T) or for the longer comparison time interval ($T + \Delta T$). The procedure was identical to that used in training (with standard = short stimulus, and comparison = long stimulus), except for the following:

After each set of eight trials, the duration of the comparison stimulus was redetermined. If 75% of the responses of the previous eight trials (i.e., six) had been correct, the duration of the comparison stimulus was unchanged. If more than 75% of the responses were correct, then the duration of the comparison signal was reduced by some constant (.2 or 1 sec). If fewer than 75% of the responses were correct, then the duration of the signal was increased by that constant.

A session consisted of 13 blocks of 8 trials (104 trials). A single standard was used for 11 successive sessions. On the first of these 11 sessions, the comparison was twice as long as the standard, and the adjustment constant was 1 sec. During the next 10 sessions, the comparison started where it left off on the previous session, and the adjustment constant was reduced to .2 sec. The last 5 of the 11 sessions were used for measurement of the difference limen.

The standards used were .5, 1, 2, 4, and 8 sec. Three determinations of each different limen were made. During the first determination an ascending order of standard durations was used. During the second determination, the order was 2, 4, 8, .5, and 1 sec. During the third determination, the three shortest durations were randomly permuted for each subject (Latin square), and then the 4- and 8-sec durations were tesed. In addition, 11 sessions were inserted following determination of the 8-sec standard. These sessions were a comparison of 1 versus 8 sec, without adjustment, to find the error rate on an extremely easy discrimination problem.

RESULTS

Error Rates on the Easy Discriminations

During the sessions of training, all three rats gradually learned to press the left lever when the short (.5 sec) signal was presented and to press the right lever when the long (8.0 sec) signal was presented. Figure 1 shows the percentage correct on all trials except the correction trials following errors.

Even during the last 10 sessions of training (after 4,900 trials) the animals did not always make the correct response. The three animals made errors on 2%, 8%, and 5% of the trials, respectively; the session with the best performance had 0%, 3%, and 2% errors.

During the 11 sessions in which the standard stimulus was 1.0 sec and the comparison stimulus was held constant at 8 sec, the rats continued to make some errors. During the last 5 of these 11 sessions the three rats made errors on 6%, 8%, and 2% of the trials, respectively; the session with the best performance had fewer than 1% errors in each case.

Determination of a Difference Limen

The results of Subject 1 during the first determination with the 8-sec standard are shown in Figure 2. This figure shows the duration of the comparison signal as a function of blocks of eight trials. The duration of the comparison signal decreased during the first session with the 1-sec gross adjustment and during the next few session with the .2-sec fine adjustment. Such gradual improvement of performance often occurred, especially with the longer standards. By the last five sessions, however, the mean setting of the comparison was usually fairly constant. In this case, it was 9.9, 10.0, 10.2, 10.2, and 9.7 sec. The median value (10.0 sec) was used for the calculation of the difference limen. Since 10.0 sec was the setting required for 75% correct responses, the difference between the setting of the comparison and the standard (10.0 − 8.0 = 2.0 sec) was defined as the difference limen. Separate values were calculated from the three determinations for each rat at each standard. Because we were primarily interested in the rats' performance when errors of inattention were at a minimum, most of the analysis concerns the lowest of these three values (the minimum difference limen in Table 1). Unless otherwise indicated, the difference limen will refer to this value.

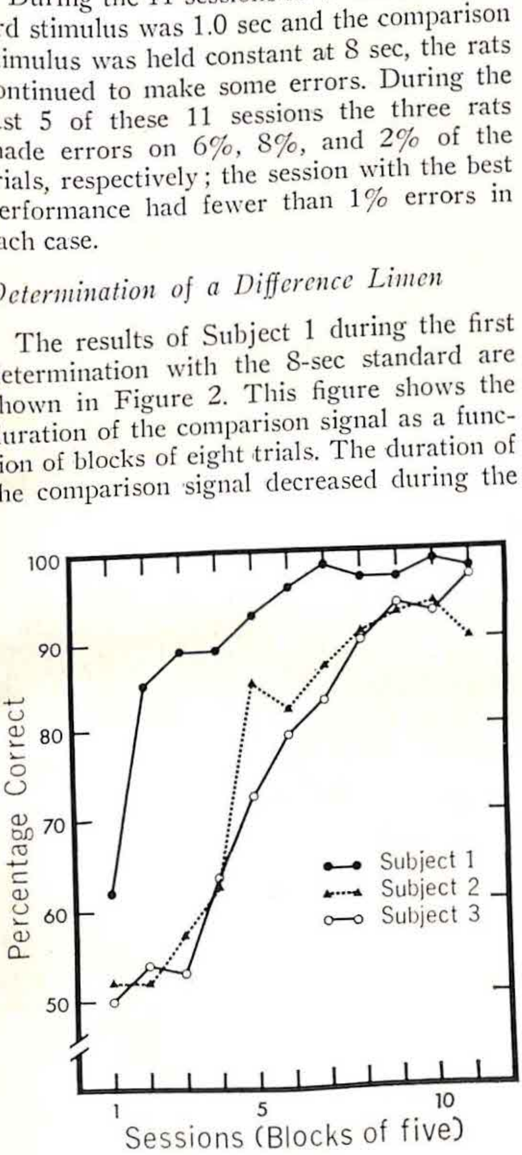

FIGURE 1. Percentage of responses correct as a function of blocks of five sessions.

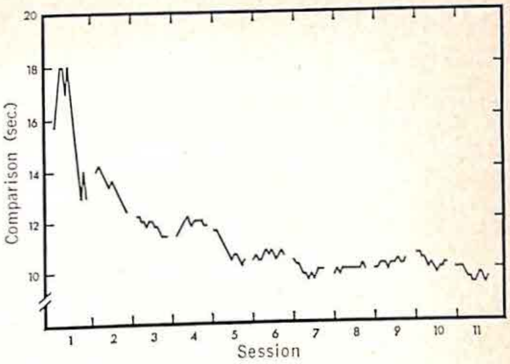

FIGURE 2. Duration of the comparison as a function of sessions for one subject. (The duration of the standard was 8.0 sec.)

Relationship Between Difference Limen and Duration of Standard

The data are from the final five sessions of each of the three determinations. Table 1 shows the minimum, mean, and standard deviation of the difference limen as a function of the duration of the standard.

Figure 3 shows the squared difference limen as a function of the standard for each of the three rats. According to the Counter

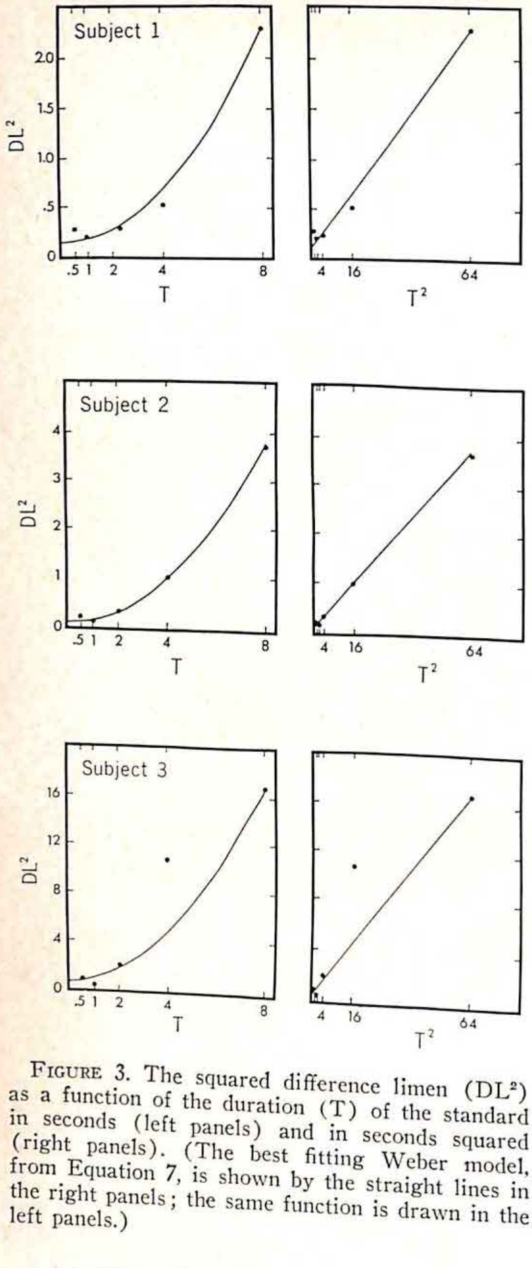

FIGURE 3. The squared difference limen (DL^2) as a function of the duration (T) of the standard in seconds (left panels) and in seconds squared (right panels). (The best fitting Weber model, from Equation 7, is shown by the straight lines in the right panels; the same function is drawn in the left panels.)

model (Equation 8, with $p = 1$) the DL^2 should be linear with T, that is, a straight line should fit the points in the left panels. According to the Weber model (Equation 8, with $p = 2$) the DL^2 should be linear with T^2, that is, a straight line should fit the points in the right panels.

A straight line has been drawn, according to the least squares criterion, through the data in the right panels of Figure 3. Such a straight line accounts for 98.7%, 99.9%, and 86.3% of the variance of the DL^2s for the three subjects. One point, the DL^2 for Subject 3 with a 4-sec standard, was inconsistently high. When this point is omitted, a straight line accounts for 99.8% of the variance. This point is omitted in the remaining analysis. If the simple version of the Weber model were true (Equation 3), a straight line beginning at the origin would fit the data in the right panels. The best estimate of the intercept for all three subjects was greater than 0 (.14, .11, and .73). Thus, we assume that there is an added source of variance (as in Equation 7).

The magnitude of the difference limen serves as a measure of overall accuracy; the slope of the line drawn through the points serves as a measure of the rate of change in accuracy as a function of increases in duration (.03, .06, .25). By either measure, one of the rats (Subject 3) discriminated durations much more poorly than the other two. (Note the different scales for the different subjects.)

The same functions shown in the right panels are redrawn in the left panels. The general impression is that they provide a reasonably good fit. To assess the Counter model (Equation 6), a straight line was fit to the data. For all three subjects, the best estimate of the intercept was negative. Since there was no meaning to a negative variance term, the value was set to 0, and the best fitting straight line was recalculated. The percentage of variance accounted for by this line was 87.1%, 89.6%, and 96.6%. For all three subjects, the amount of variance not accounted for by the Counter model was significantly greater than the amount of variance not accounted for by the Weber model, $Fs(3, 3) = 9.8, 131.8$, and 19.0, $p < .05$.

The Weber fraction may be defined as the difference limen divided by the standard. If the simple version of the Weber model were true (Equation 3), the Weber fraction would be a constant, unrelated to the duration of the standard; if the version of the Weber model with added variance were true (Equation 7), the Weber fraction would decrease as the standard increases. Figure 4 shows the

TABLE 1

DIFFERENCE LIMEN AS A FUNCTION OF DURATION OF THE STANDARD

Time (in sec)	Subject 1			Subject 2			Subject 3		
	Min	*M*	*SD*	Min	*M*	*SD*	Min	*M*	*SD*
.5	.52	.63	.16	.44	.62	.24	.92	1.09	.31
1.0	.46	.65	.21	.39	.49	.18	.75	1.08	.33
2.0	.48	.94	.40	.54	.72	.21	1.46	1.88	.43
4.0	.72	1.17	.39	1.00	1.41	.52	3.28	3.46	.38
8.0	1.52	1.68	.35	1.94	2.31	.42	4.11	5.14	.89

Note. Min = minimum.

latter is the case. The solid theoretical function is the Weber model with the constants obtained previously. The predicted asymptotic Weber fractions were .18, .24, and .50.

Although the generalized version of the Weber model (Equation 7) fits the data better than the generalized version of the Counter model (Equation 6), there is reason to consider the possibility that the exponent of Equation 8 is neither 1 nor 2. Figure 5 shows the percentage of variance accounted for by different values of the exponent of Equation 8 for Subject 2. The estimates of the exponent of Equation 8 that minimized the sum of the squared deviations of the points from the line were 2.9, 2.1, and 1.7 for the three rats. In only one case (Subject 1) was the amount of variance not accounted for by the Weber model significantly more than that not accounted for by the optimal value of the exponent. In that case, Equation 8 with an exponent of 2.9 accounted for 99.9% of the variance, but Equation 8 with an exponent of 2.0 (The Weber model) was quite satisfactory. It accounted for 98.7% of the variance.

Observations of Behavior

We watched the animals during some sessions throughout the experiment. They showed considerable enthusiasm for a task which, for a person, would be an extremely dull and tedious psychophysical effort. When the levers were being inserted into the box, the animals typically pounced upon one of them. Although latencies were not recorded, those we observed were all rapid (well under a second). During the intertrial interval and during the signal, each animal exhibited sterotyped behavior. Notes from one session are as follows: Subject 1 bit the front of the left lever during the intertrial interval and the front of the right lever when the signal was on. (The levers, of course, were withdrawn during the intertrial interval.) Subject 2 sat near the left level during the in-

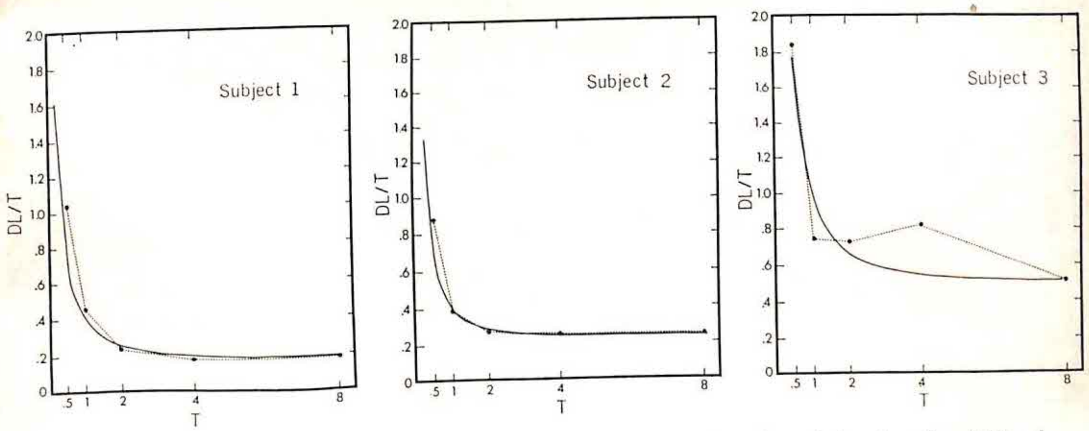

FIGURE 4. The Weber fraction (DL = difference limen) as a function of the duration (T) of the standard in seconds for the three subjects. (The solid line is based upon the best fitting Weber model, from Equation 7.)

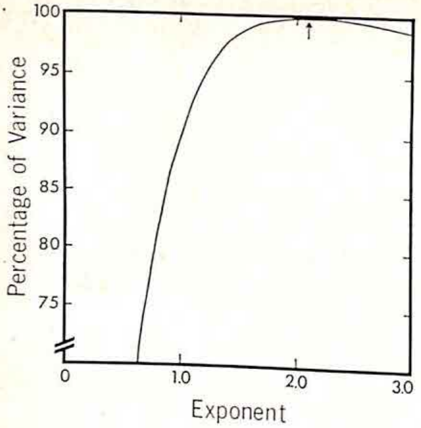

FIGURE 5. The percentage of variance accounted for by Equation 8 with various exponents. (The maximum is indicated by an arrow. Data are from Subject 2.)

tertrial interval, mouthing both paws. When the signal was on, it moved from the right lever to the cup, back and forth rapidly. Subject 3 made head movements into the food cup during the intertrial interval and continued to do so when the signal was on. We noted that the rhythmic, stereotyped behavior of the two subjects with better discrimination (Subjects 1 and 2) was more rapid than that of the subject with poorer discrimination (Subject 3) and also that the behavior during the signal was the same as the behavior during the intertrial interval only for the subject with the poorer discrimination. The particular behaviors changed somewhat throughout the experiment, but these rules remained valid.

DISCUSSION

The results are consistent with a theory of duration discrimination that recognizes three limitations to an animal's accuracy in estimating temporal duration: (a) inattention to the signal on some trials, (b) variability in starting to time the duration when the signal begins and/or in stopping to time the duration when the signal ends, and (c) factors related to signal duration itself.

Inattention

On the easy discrimination (.5 vs. 8 sec, and 1 vs. 8 sec) the animals made some errors. The mean percentage correct on these conditions can be used as a basis for estimating the percentage of trials on which the animal was paying attention to the task. Our assumption is that any errors made on such a task are due to lack of attention (i.e., that $P_a = 1$ in Equation 1). Thus, it is possible to solve for a in Equation 1 to find the percentage of trials on which the three animals were paying attention to the signal.

On the more difficult discriminations used during testing, the up-down psychophysical procedure guaranteed that the animals would be correct on about 75% of the trials. The probability that they were correct on those trials on which they were paying attention to the signal, of course, would be somewhat higher, and the value can be found by substitution in Equation 1. With some further assumptions, for example, a normal psychometric function, it would be possible to adjust the observed difference limens to correct for inattention. We chose to use a simpler approach. The measure of the difference limen we used was the minimum of three determinations, and it presumably was a case in which the animals were sufficiently attentive that no downward adjustments were required.

Signal Onset and Termination

On those trials in which the animal was paying attention, the time from signal onset until the animal began timing, and the time from signal termination until the animal stopped timing, were random variables independent of signal duration. The influence of this added source of variability is seen most clearly by the positive intercepts of Figure 3 and by the decrease in the Weber fraction shown in Figure 4. Without this added source of variance, the intercept of Figure 3 would pass through the origin, and the Weber fractions would be constant.

Factors Related to Signal Duration

The difference limen increased as the duration of the standard increased. The form of this increase can be expressed by Equation 8. The generalized Counter model led to the expectation that the exponent of Equation 8 would be 1.0; the generalized Weber model led to the expectation that the exponent would be 2.0. In fact, an exponent of 2.0 was reliably better than an exponent of 1.0. This provides support for the generalized Weber model (Equation 7).

Method of Production

The data from the present experiment are similar to those obtained by Platt, Kuch, and Bitgood (1973) in a psychophysical task in which rats produced durations. In that experiment depression of a lever for more than a criterion duration was reinforced. The criterion durations were between .4 and 6.4 sec, and the interquartile range of the press durations (in sec) was reported. Figure 6 replots the mean of five rats in the form strictly analogous to Figure 3. There are several reasons to prefer the Weber model to the Counter model: (a) The Counter model (left panel, Figure 6) accounted for 90.4% of the variance, while the Weber model (right panel) accounted for 99.9% of the variance; (b) the best estimate of the exponent of Equation 8 is 2.0; and (c) the amount of variance not accounted for by the Counter model is reliably greater than the amount of variance not accounted for by the Weber model, $F(3, 3) = 49.3$, $p < .01$. The variance attributable to the time from signal onset until the subject begins timing might be lower in the production method than the discrimination method (since in the production method the subject has control of the time of signal onset); the variance attributable to the time from signal termination until the subject stops timing might be higher in the production method than the discrimination method (since in the production method there is variability due to motor factors). Several previous studies have explicitly recognized that timing may not begin until a motor response has been completed (Libby & Church, 1974; Gibbon, 1971), but this has not been formally introduced as an added variance component. Although Platt, Kuch, and Bitgood were impressed by the relative constancy of the Weber fractions, the first point was the highest (actual value not reported), and the remaining points decreased in order (.28, .23. .23, and .22). On balance, the actual level of duration discrimination as well as the functional form seems to be quite similar in the production method (e.g., Platt, Kuch, and Bitgood, 1973; Catania, 1970; Cantor, Note 1) and in the discrimination method. In both cases the difference limen was well described by a generalized Weber model (Equation 7).

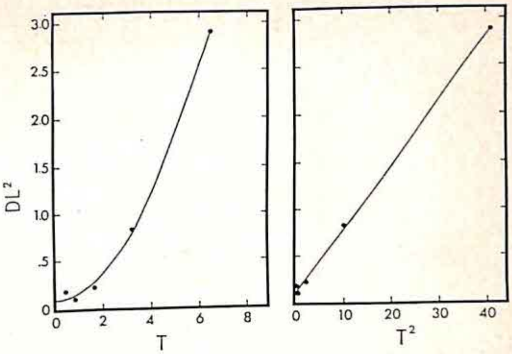

FIGURE 6. The squared difference limen (DL^2) as a function of the duration (T) of the standard. (The form of this figure is equivalent to that of Figure 3. Data are from Platt, Kuch, and Bitgood, 1973.)

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Surprise and the Attenuation of Blocking

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Three experiments, employing conditioned suppression in rats, examined the extent to which pretraining on one element of a compound stimulus blocked conditioning to the other element when some feature of the reinforcer was changed on compound trials. In Experiment 1, both the addition of an unexpected second shock 8 sec after the end of each trial and the postponement of an expected second shock from 4 to 8 sec attenuated blocking. Experiment 2 established that this latter unblocking effect was due to the surprising postponement of the second shock on trials when the new element was added rather than to any generalization decrement from pretraining to compound training. Experiment 3 showed that blocking was attenuated not only by the addition of an unexpected second shock but also by the omission of an expected shock.

In a series of experiments on conditioned suppression in rats, Kamin (1968, 1969) showed that the amount of conditioning accruing to one element of a compound conditioned stimulus (CS) over a series of reinforced compound trials was affected by the subject's prior experience with the other element. If one element had previously been paired with shock, it might severely attenuate or, in Kamin's words, block conditioning to the other element.

Kamin interpreted his results to mean that conditioning was dependent on the predictability of reinforcement: No conditioning accrued to the added element on compound trials because reinforcement was already fully predicted by the presence of the pretrained element. Only surprising reinforcers are reinforcing. Rescorla and Wagner (1972) have proposed a model of Pavlovian conditioning which is intended to capture at least part of this idea. They assume that a given reinforcer can support only a certain amount of conditioning (represented by a parameter, λ); and increments to the associative strength of one element of a compound CS are proportional to $(\lambda - \overline{V})$, where $\overline{V}$ represents the current associative strength of all stimuli present, including, of course, other elements of the compound CS. In the blocking experiment, sufficient initial training with one CS will drive its associative strength close to λ, with the consequence that on compound trials the quantity $(\lambda - \overline{V})$ will be very small and the added element can gain little or no associative strength.

Kamin (1969) presented further experimental evidence that is entirely consistent with these accounts of blocking. He showed, for example, that significant conditioning would occur to the added element (i.e., blocking would be attenuated) if the compound CS signaled a stronger shock than that paired with the first element during pretraining. The stronger shock was unpredicted or surprising and hence acted as an effective reinforcer. In Rescorla and Wagner's terms the amount of conditioning supportable by a strong shock will be greater than that supported by a weak shock, and with an increase in λ the quantity $(\lambda - \overline{V})$ will be sufficiently large to enable the added element to gain a significant amount of conditioning.

A second experiment reported by Kamin, however, although not necessarily creating difficulties for Rescorla and Wagner's model, may at least suggest that their analysis is

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not identical to Kamin's. In this experiment, Kamin produced significant conditioning to the added element, not by increasing the intensity of shock, but by programming a second, surprising shock 5 sec after each compound trial. As this double shock did not produce more rapid initial conditioning than a single shock, Kamin argued that this second case of unblocking could not be attributed to an increase in the strength of reinforcement. Whether or not one accepts this argument (and Rescorla and Wagner, 1972, p. 79, do not), it serves to pinpoint the difference between Kamin's and Rescorla and Wagner's analyses. According to Kamin, unblocking may require only that the added element signal some surprising event; according to Rescorla and Wagner, significant excitatory conditioning to the added element appears to require an increase in the magnitude of reinforcement on compound trials. The present experiments sought to distinguish between these two possibilities by scheduling changes in reinforcement on compound trials that could not plausibly be regarded as an increase in magnitude.

EXPERIMENT 1

That the addition of a second, delayed shock on compound trials produces some conditioning to the added element is not sufficient to decide between Kamin's and Rescorla and Wagner's analyses, since an additional shock, even if delayed, presumably constitutes some increase in total aversive stimulation and may be regarded as increasing the overall magnitude of aversive reinforcement. In Experiment 1, therefore, we compared the effects of adding a second, surprising shock on compound trials to those produced by simply *delaying* by a few seconds the delivery of an already expected second shock. One group of rats was initially trained with a CS signaling a single shock, while a second group received comparable training except that a second shock was delivered 4 sec after the end of each conditioning trial. Both groups then received a series of compound trials on which a second shock was delivered 8 sec after the end of each trial. The first group constitutes, of course, a partial replication of Kamin's experiment; the second should show whether postponing the delivery of a second shock, an operation unlikely to constitute an increase in the magnitude of effective reinforcement, is sufficient to produce unblocking. A third, control group received a second shock, delayed by 8 sec on all trials during both initial and compound training, while a fourth group was given no initial training with one element in order to provide some assessment of the magnitude of the blocking and unblocking effects observed in the other three groups.

Method

Subjects and apparatus. The subjects were 32 male hooded rats from the colony maintained at the University of Sussex, weighing approximately 300 g at the start of the experiment. They were housed in individual cages with unlimited access to water, and gradually reduced to 80% of their free-feeding weights. They were maintained at this weight for the remainder of the experiment, being fed an appropriate number of pellets immediately after each day's training session.

The apparatus consisted of four two-lever rat chambers manufactured by Campden Instruments. The right-hand lever was removed from the chambers, and a white Perspex ceiling substituted for that usually fitted. A 60-W, 240-V strip light was mounted above the Perspex ceiling. The chambers were placed inside sound-attenuating shells, and the experiment was controlled by automatic programming equipment in an adjoining room.

Throughout each session, the chambers were permanently illuminated by a 24-V white jewel light mounted in the center of the front wall, and white masking noise was delivered from a speaker mounted in the center of the rear wall. Two CSs were used, a light and a clicker. The light was produced by switching on the overhead strip light, the clicker by a Campden Instruments audio generator delivering a 10-Hz train of clicks to the speaker. All CSs were 100 sec long. Shocks were delivered by Grason-Stadler shock generators and scramblers.

Procedure. The preliminary phase of the experiment involved magazine training and the establishment of lever pressing. On Day 1, subjects received a 50-min session, with the lever removed, and free food (45-mg pellets) delivered on a variable-time 1-min schedule. On Day 2, the lever was inserted, and lever pressing was consistently reinforced. This session lasted 30 min. Rats failing to eat pellets on Day 1 or to press the lever on Day 2 were given additional training. Throughout the remainder of the experiment, all sessions were 50 min long. On Day 3, lever pressing was reinforced on a variable-interval (VI) 30-sec schedule, and on Days 4 and 5 (and for the remainder of the ex-

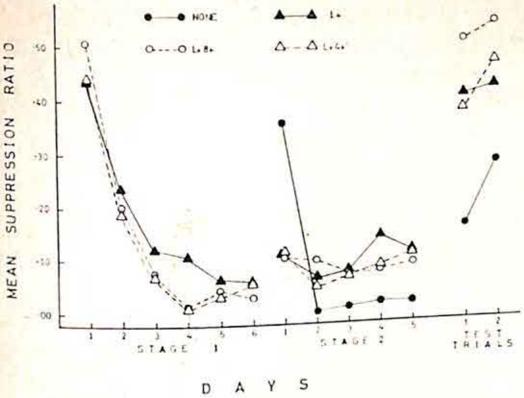

FIGURE 1. Experiment 1: Mean suppression ratios to the light during Stage 1, to the clicker–light compound during Stage 2, and to the clicker during the test trials. (None = no conditioning trials during Stage 1; L+ = light paired with single shock during Stage 1; L+4+ = light paired with double shock during Stage 1 with 4-sec intershock interval; L+8+ = light paired with double shock during Stage 1 with 8-sec intershock interval.)

periment) on a VI 1-min schedule. On Days 6 and 7 all subjects received nonreinforced pretest trials to the light and to the clicker. On each day there were two trials, beginning 20 and 37 min after the beginning of the session, during which either the light or the clicker was presented without programmed consequence.

Conditioning sessions also contained two trials, beginning at the 20th and 37th min after the start of the session. On each trial, the 100-sec CS terminated with a .5-sec 1.0 mA grid shock, and, where scheduled, a second shock of the same duration and intensity was delivered 4 or 8 sec after the end of the first. There were 12 conditioning trials in Stage 1, and 10 in Stage 2, followed by two nonreinforced test trials. In Stage 1, Group L+ was conditioned to the light signaling a single shock, while for Groups L+4+ and L+8+, a second shock was delivered 4 or 8 sec after the first. Group N received no conditioning trials in Stage 1, but continued to lever press on the VI 1-min schedule. In Stage 2, all groups received 10 compound (clicker and light) trials, each followed by two shocks with an 8-sec interval between first and second shocks. On the test day, all groups received two nonreinforced trials with the clicker.

Results

Response suppression during the CS was expressed as a suppression ratio to attenuate the effects of individual differences in the overall rate of responding. The ratio has the form $A/(A + B)$ where A is the rate of responding during the CS and B the rate during a 100-sec period immediately prior to CS onset.

On pretest trials the group mean suppression ratios to the light ranged from .40 to .47, $F < 1$, and to the clicker from .41 to .53, $F(3, 28) = 1.55$, $p > .10$.

Figure 1 portrays the mean suppression ratios of the different groups to the light during Stage 1, to the clicker–light compound during Stage 2, and to the clicker during the test trials. Inspection of Figure 1 suggests that the groups receiving a double shock (Groups L+8+ and L+4+) during Stage 1 suppressed more rapidly than the group receiving a single shock (Group L+). However, this conclusion was not supported by statistical analysis. Neither the effect of groups ($F < 1$) nor the Groups × Days interaction ($F < 1$) was significant. An examination of pre-CS response rates, however, suggested that Group L+8+ responded, at least on some days, at a slower rate than the remaining groups. An analysis of variance performed on these pre-CS rates revealed a significant Groups × Days interaction, $F(10, 105) = 3.36$, $p < .01$, and a separate analysis of response rates on each session showed a significant effect of groups on Session 3, $F(2, 21) = 7.01$, $p < .01$, and Session 5, $F(2, 21) = 11.21$, $p < .01$. Individual comparisons using a Newman-Keuls procedure indicated that Group L+8+ responded significantly more slowly than Groups L+ and L+4+ on both these sessions ($p < .05$).

In Stage 2 the three pretrained groups maintained comparable and constant levels of suppression. Group N rapidly developed suppression to the compound CS and after the first session suppressed to a greater extent than the other three groups. An analysis of suppression ratios revealed a significant Groups × Trials interaction, $F(12, 112) = 11.95$, $p < .01$. Separate analyses showed a significant effect of groups on all sessions ($p < .05$ in all cases), with Group N being significantly less suppressed than the other three groups on the first session and significantly more suppressed than Group L+ on Sessions 3, 4, and 5, than Group L+4+ on Sessions 3 and 5, and than Group L+8+ on Sessions 2 and 3 ($p < .05$ in all cases).

There was no significant effect of groups on the pre-CS response rates during Stage 2, $F(3, 28) = 2.33$, $p > .05$.

During the test trials the basic blocking effect was demonstrated by the fact that Group N was more suppressed than Group L+8+. The absence of suppression in Group L+8+ suggests that the blocking of conditioning to the clicker was almost complete. The intermediate levels of suppression maintained by Groups L+ and L+4+ show that both adding and delaying a second shock attenuated blocking to about the same extent without completely abolishing it. An analysis of suppression on the test trials revealed a significant effect of groups, $F(3, 28) = 17.35$, $p < .01$. Individual comparisons showed that Group N was significantly more suppressed than the three remaining groups ($p < .01$ in all cases), and that Groups L+ and L+4+ were more suppressed than Group L+8+ ($p < .05$ in both cases). The pre-CS response rates of the four groups did not differ on test trials, $F(3, 28) = 1.53$, $p > .10$.

Discussion

The results of Experiment 1 confirm Kamin's (1969) finding that the addition of a second shock during compound training attenuates blocking. More importantly, it appears that simply delaying the second shock produces a comparable degree of unblocking. Although it might be argued that the addition of a second shock could have increased the effective magnitude of the aversive reinforcer and so permit an explanation of unblocking in terms of Rescorla and Wagner's (1972) theory, it is rather less likely that postponing the second shock could have had such an effect. There is good evidence that the longer the delay between CS termination and unconditioned stimulus onset the more slowly conditioning occurs (e.g., Kamin, 1965); it is not entirely plausible, therefore, to argue that a change from a 4- to 8-sec intershock interval could have represented an increase in the total reinforcement magnitude. Moreover, although the comparison is made difficult by a difference in pre-CS response rates, there is no suggestion that Group L+8+ suppressed more rapidly in Stage 1 than Group L+4+.

Even if it is accepted that postponing the second shock from 4 to 8 sec could not have increased the overall magnitude of reinforcement, there is one possible interpretation of these results that makes no appeal to surprise as a cause of unblocking. In an earlier attempt to show that surprise was the important factor, Feldman (1971) studied blocking in appetitive instrumental discrimination learning by rats, changing from a consistent schedule of reinforcement in initial training with one element to a partial schedule of reinforcement during compound training. In spite of the consequent reduction in the overall probability of reinforcement correlated with the addition of the new element, Feldman found that it gained significant control over his rats' behavior, and attributed this unblocking effect to the surprising change in reinforcement. Neely and Wagner (1974), however, suggested an alternative interpretation in terms of generalization decrement. The change in reinforcement schedule, they argued, might have disrupted the conditioning produced by initial training, and thus, in Rescorla and Wagner's (1972) terms, have increased the magnitude of the quantity $(\lambda - \overline{V})$, not by increasing λ but by decreasing $\overline{V}$. In the context of Feldman's experiment this is a plausible suggestion, for it amounts to saying that at the end of initial training, the subjects' behavior was controlled not only by the experimenter's nominal discriminative stimulus but also by stimuli arising from the schedule of reinforcement, such as traces of the outcomes of previous trials. A change in schedule of reinforcement will alter this latter class of stimuli and therefore, by generalization decrement, reduce $\overline{V}$.

It does not, perhaps, seem very probable that in the present experiment a change from a 4-sec to an 8-sec interval between two shocks on one trial should have significantly disrupted conditioning on a subsequent trial occurring not less than 17 min later. Nevertheless, the argument is, in principle, important enough to be taken seriously.

EXPERIMENT 2

Neely and Wagner (1974) were able to show that generalization decrement was the cause of the unblocking observed in Feldman's (1971) experiment by running additional experiments in which trials to the compound stimulus signaling a changed schedule of reinforcement were interspersed with trials to the pretrained component alone still correlated with its original schedule of reinforcement. By thus maintaining the traces of the initial schedule, this procedure should have prevented significant generalization decrement. It also abolished the unblocking effect.

Experiment 2 was designed first to confirm the finding of Experiment 1 that the shift from a 4-sec to an 8-sec interval between shocks was sufficient to produce unblocking, and secondly, to see whether generalization decrement was in any way responsible for this unblocking effect. After conditioning in Stage 1 to the light alone paired with a double shock, all subjects received two trials per session in Stage 2, the first trial on each day being to the light alone and the second to the clicker–light compound. A factorial design made it possible to assess separately the effects of a shift in intershock interval associated with compound trials and one associated with interspersed trials to the light alone. An analysis in terms of surprise predicts that unblocking would depend on a shift in the intershock interval on compound trials, while the principle of generalization decrement implies that a shift in the intershock interval on each preceding trial to the light alone would produce unblocking.

Method

The subjects were 32 male rats from the same stock as those used in Experiment 1, and the apparatus and general procedure were both exactly the same as before. The magazine and lever-press training was the same as in Experiment 1 except that subjects received only two sessions of training on the VI 1-min schedule. Subsequently four pretest sessions were administered, two with the light and two with the clicker. Each session contained only one stimulus presentation.

TABLE 1

Design of Experiment 2

Group	Treatment Stage 1, 3 sessions	 Stage 2, 8 sessions	 Test, 4 sessions
S	2L+4+	L+4+, CL+8+	L+4+, C
C	2L+8+	L+8+, CL+8+	L+8+, C
S–GD	2L+4+	L+8+, CL+8+	L+8+, C
C–GD	2L+8+	L+4+, CL+8+	L+4+, C

Note. Abbreviations: S = surprise, *C* = control, GD = generalization decrement; L = light, C = clicker, +4+ = double shock with 4-sec intershock interval, +8+ = double shock with 8-sec intershock interval.

The design of the experiment is shown in Table 1. As was the case for Groups L+4+ and L+8+ in Experiment 1, all subjects were conditioned to the light signaling a double shock in Stage 1 (with either a 4-sec or an 8-sec interval between shocks), and in Stage 2 they were all conditioned to the clicker–light compound signaling a double shock with an 8-sec interval between shocks. In an attempt to increase the overall level of conditioning to the clicker, only 6, rather than 12 trials were given to the light in Stage 1. There were 8 compound trials in Stage 2, but these were spread over eight days as the second trial of each day. The first trial of each day in Stage 2 was to the light alone. For Groups S and *C* (surprise and control), the shock signaled by the light alone was exactly the same in Stage 2 as in Stage 1; for Group *C*, of course, the compound also signaled the same shock as the light, while for Group S, the compound signaled a longer interval between shocks. For Groups S–GD and *C*-GD (surprise–generalization decrement, and control–generalization decrement) the light signaled a different interval between shocks in Stage 1 and Stage 2. The intershock interval associated with compound trials was the same as that experienced in Stage 1 for Group *C*-GD, but longer for Group S-GD.

All subjects received four test trials to the clicker alone, one on each of four test days. On each day, the nonreinforced clicker trial was preceded, as in Stage 2, by a trial to the light alone signaling the same pair of shocks as in Stage 2.

Results

On pretest trials the group mean suppression ratios to the light ranged from .33 to .42, $F(3, 28) = 2.05$, $p > .10$, and to the clicker from .39 to .46, $F < 1$.

Figure 2 illustrates the acquisition of suppression to the light in Stage 1, suppression to the clicker–light compound in Stage 2, and the suppression controlled by the clicker alone on test trials. During Stage 1, Group S–GD appeared to condition more slowly to the light than the remaining three groups. This difference cannot be due to the training conditions in effect during Stage 1, since Group S–GD was treated in exactly the same way as Group S (both receiving a 4-sec intershock interval). It presumably reflects a sampling error. By the end of Stage 1, all

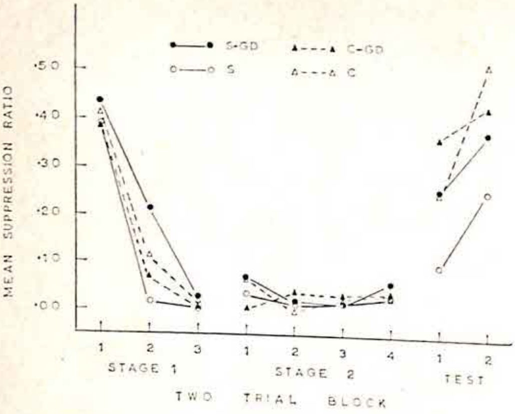

FIGURE 2. Experiment 2: Mean suppression ratios to the light during Stage 1, to the clicker–light compound during Stage 2, and to the clicker alone during the test trials. (S = surprise; S-GD = surprise and generalization decrement; C = control; C-GD = control and generalization decrement.)

groups showed nearly complete suppression to the light. An analysis over all three days showed a significant effect of groups, $F(3, 28) = 4.12$, $p < .025$, but no significant Groups × Days interaction, $F(6, 56) = 1.93$, $p > .05$. Individual comparisons (Newman-Keuls test) showed that Group S–GD was less suppressed than Groups S and *C*–GD, $p < .05$ in each case, while Groups *C*, *C*–GD, and S could not be differentiated. Pre-CS response rates did not differ between groups; for both the main effect of groups and the Groups × Days interaction, $F < 1$.

All groups maintained virtually complete suppression both to the light alone and to the clicker–light compound throughout Stage 2. Suppression on trials to the light alone was affected neither by whether the intershock interval was 4 or 8 sec, nor by whether the interval had been changed from Stage 1 to Stage 2, nor by the interaction of these factors, $F < 1$ in all cases. Suppression to the clicker–light compound was similarly unaffected by changing the intershock interval associated with the compound (surprise factor), or that associated with the light alone (generalization decrement), or by their interaction, $F < 1$ in all cases. Finally, there were no differences between the four groups in their pre-CS response rates, $F(3, 28) = 1.33$, $p > .10$.

Inspection of Figure 2 shows that the two surprised groups exhibited greater suppression to the clicker than their respective controls throughout the test trials. By contrast, the effect of generalization decrement interacted with test trials. On Trials 1 and 2 the groups experiencing generalization decrement showed less suppression than their respective controls. Although this difference was preserved on Trials 3 and 4 for Groups S–GD and S, it was reversed for the two nonsurprised groups, with Group *C* exhibiting less suppression than Group *C*–GD. There was no difference between the four groups in the level of suppression maintained on retraining trials to the light on these test days.

These conclusions were confirmed by statistical analysis. An overall analysis of suppression to the clicker revealed a significant effect of surprise, $F(1, 28) = 12.91$, $p < .01$, and a significant Generalization Decrement × Trials interaction, $F(3, 84) = 3.19$, $p < .05$. Neither the main effect of generalization decrement, $F(1, 28) = 3.58$, $p > .05$, nor the Surprise × Generalization Decrement interaction, $F(1, 28) = 2.44$, $p > .10$, reached significance. An analysis of performance on individual test trials revealed a significant effect of generalization decrement on Trial 2, $F(1, 28) = 6.34$, $p < .025$, and a significant Surprise × Generalization Decrement interaction on Trial 3, $F(1, 28) = 10.28$, $p < .01$. An analysis of suppression on trials to the light alone showed that performance was affected neither by the intershock interval associated with the light, nor by whether this interval had been shifted from Stage 1 to Stage 2, nor by their interaction, $F < 1$ in all cases. The four groups showed similar pre-CS response rates throughout testing, $F < 1$.

Discussion

The results of Experiment 2 agree with those of Experiment 1 in showing that the postponement of a second shock on compound trials may attenuate blocking. They also demonstrate that generalization decrement in no way contributes to this result. If the attenuation of blocking were entirely attributable to generalization decrement, then

Group S would have shown no more suppression to the added element than Group *C*. This was clearly not so. If generalization decrement were even partially responsible for the attenuation of blocking, then Groups S–GD and *C*–GD would have shown more suppression to the clicker than Groups S and *C*, respectively. There was, however, no consistent difference between Groups *C*–GD and *C*, while Group S–GD was reliably less suppressed than Group S.

This latter difference is consistent with the suggestion that the unblocking observed here is entirely a consequence of correlating clicker–light trials and light-only trials with different intershock intervals. The explicit method of producing surprise in this experiment was to change the intershock interval correlated with compound trials in Stage 2 from that correlated with light trials in Stage 1. Blocking should also be attenuated, however, by a difference between the intershock intervals correlated with light and clicker–light trials within Stage 2 itself. Since Group S was exposed to such a difference while Group S–GD was not, the greater suppression to the clicker shown by Group S is further evidence of the importance of surprise.

Groups *C* and *C*–GD both received the same intershock interval on compound trials as they had on light trials in Stage 1, but for Group *C*–GD the intershock interval on light trials was changed in Stage 2. There is no obvious reason why Group *C* should have shown more suppression on early test trials but less on later trials, and it does not seem worth speculating further on this pattern of results.

There were no reliable differences between the various groups in suppression to the light in Stage 1. and no suggestion of any difference between groups receiving the 4-sec intershock interval and those receiving the 8-sec interval. This again suggests that the attenuation of blocking we have observed cannot be a consequence of any increase in the effective magnitude of reinforcement. Even if this is accepted, however, it remains true that the surprising postponement of a second shock must involve the presentation of an unexpected shock. It might be possible, therefore, for Rescorla and Wagner's (1972) model to explain the unblocking observed in Experiments 1 and 2, not by arguing that a longer delay between two shocks was generally more reinforcing than a shorter delay but rather by supposing that the second shock would be more reinforcing if it occurred at an unexpected time rather than at an expected time. It could be assumed that temporal cues dating from the termination of the CS would come to signal the occurrence of the second shock and serve to block further conditioning to that shock, provided that it continued to occur at the same interval after the CS. Any change in the time of the second shock, however, would mean that its occurrence was not fully predicted and would enable it to reinforce further conditioning.

EXPERIMENT 3

Although these arguments are somewhat speculative, the point they raise is sufficiently important to justify further experimental analysis. The one condition that could be construed neither as increasing the overall magnitude of reinforcement nor as involving the presentation of an unexpected second shock would be the omission of an otherwise expected second shock. Experiment 3 employed a simple factorial design in which the CS signaled either a single shock or a pair of shocks in Stage 1, and each of these groups was divided in Stage 2 for compound training, with half receiving a single shock following each compound trial and half receiving a double shock. For one group, therefore, the new element added on compound trials signaled the addition of a second shock, and for another it signaled the omission of a second shock. The two control groups received the same number of shocks, either one or two, in both stages of the experiment. To add further generality to our results, the stimulus used in Stage 1 was the clicker, and the stimulus added in Stage 2 was the light.

Method

The subjects were 32 male rats from the same stock as those used in Experiments 1 and 2. The apparatus and general procedure were the same as before.

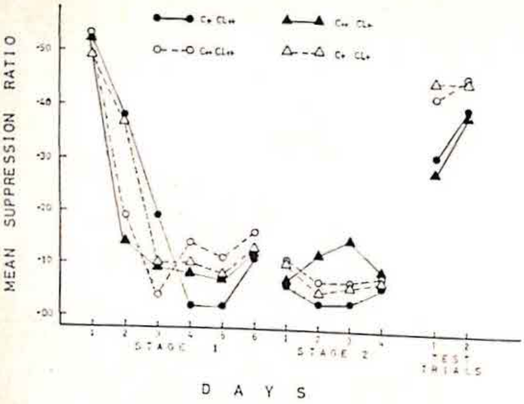

FIGURE 3. Experiment 3: Mean suppression ratios to the clicker (C) during Stage 1, to the clicker–light (CL) compound during Stage 2, and to the light (L) during the test trials. (C+ CL++ = single shock during Stage 1 and double shock during Stage 2; C++ CL++ = double shock during Stages 1 and 2; C++ CL+ = double shock during Stage 1 and single shock during Stage 2; C+ CL+ = single shock during Stages 1 and 2.)

In Stage 1, all subjects received two conditioning trials to the clicker (C) on each day for 6 days. For Group C+ each trial terminated with a single shock; for Group C++, a second shock was delivered 8 sec after the first. In Stage 2 there were two reinforced trials each day to the clicker–light compound (CL) for four days. Groups C+ and C++ were each subdivided, and Groups C+ CL+ and C++ CL+ received a single shock on each trial, while Groups C++ CL++ and C+ CL++ received a second shock on each trial 8 sec after the first. On the final day of the experiment there were two nonreinforced test trials to the light.

Results

On pretest trials the group mean suppression ratios to the light ranged from .40 to .44, $F < 1$, and to the clicker from .44 to .48, $F < 1$.

During Stage 1 the groups receiving two shocks developed suppression more rapidly to the clicker than the groups receiving a single shock. This difference is illustrated in Figure 3, which portrays the suppression to the clicker during Stage 1, to the clicker–light compound during Stage 2, and to the light on test trials. Although there was no overall significant effect of the number of shocks during Stage 1, $F(1, 30) = 2.44$, $p > .10$, the Number of Shocks × Days interaction was significant, $F(5, 150) = 5.84$, $p < .01$. Separate analyses of suppression on each day of Stage 1 showed that Groups C++ CL+ and C++ and CL++ were significantly more suppressed than Groups C+ CL+ and C+ CL++ on Day 2, $F(1, 30) = 20.20$, $p < .01$. An analysis of pre-CS response rates showed that neither the main effect of number of shocks, $F < 1$, nor the Number of Shocks × Days interaction, $F(5, 150) = 1.26$, $p > .25$, reached significance.

In Stage 2 Groups C+ CL+ and C++ CL++, which were not surprised by either the addition or omission of a shock, maintained comparable levels of suppression to the compound CS. During Days 2 and 3 the group surprised by the addition of a shock, Group C+ CL++, tended to be slightly more suppressed than the nonsurprised groups, while Group C++ CL+, surprised by the omission of a shock, was slightly less suppressed. However, by the end of Stage 2 these small differences had completely disappeared. Although neither the main effect of surprise, ($F < 1$), nor the effect of number of shocks in Stage 2, $F(1, 28) = 3.09$, $p > .05$, significantly affected suppression, the Surprise × Number of Shocks interaction, $F(1, 28) = 4.45$, $p < .05$, was significant. There was no evidence that the pre-CS response rate varied as a function of surprise or shock number. The F ratios for both these effects and for the Surprise × Number of Shocks interaction were all less than 1.

The two groups surprised during Stage 2, Groups C+ CL++ and C++ CL+, both exhibited more suppression to the light on test trials than the nonsurprised groups, Groups C+ CL+ and C++ CL++. However, the level of suppression did not vary as a function of the number of shocks received during Stage 2. There was a significant effect of surprise, $F(1, 28) = 16.16$, $p < .01$, but the F ratios for the effect of number of shocks and the Surprise × Number of Shocks interaction were both less than 1. An analysis of the pre-CS response rates revealed no significant effects with all F ratios less than 1.

Discussion

The results of this experiment provide no support for the suggestion that attenuation of blocking requires an increase in the effective magnitude of reinforcement from Stage 1 to Stage 2. The differences observed in the acquisition of suppression in Stage 1 suggested that, if anything, a single shock acted as a less effective reinforcer than a double shock. Nevertheless, the change from a double shock in Stage 1 to a single shock in Stage 2 produced just as much unblocking as the addition of a second shock.

GENERAL DISCUSSION

In Kamin's (1968, 1969) blocking experiments, little or no conditioning accrued to one element of a compound CS, if its addition to the compound signaled no change in reinforcement from that already predicted by the first element. If the new element signaled an increase in shock intensity, or the addition of a second shock shortly after the first, then it would acquire significant associative strength. The present results suggest that it is not necessary that the new element should signal any increase in shock, for either the temporary postponement of an expected second shock or its complete omission was as effective as the addition of an unexpected shock in producing conditioning to the new element. Rescorla and Wagner (1972) predict that any excitatory conditioning to the new element beyond the level found in control groups must depend either on a significant increase in the overall magnitude of reinforcement on compound trials or on the occurrence of an otherwise unpredicted shock. The results of Experiment 3 are particularly hard to reconcile with this analysis. The omission of an expected second shock can hardly be regarded either as constituting an increase in the magnitude of reinforcement or as equivalent to the presentation of an unpredicted shock. The critical finding of Experiment 3 appears sufficiently reliable, since we have been able to replicate it in a somewhat different experimental situation (Mackintosh, Bygrave, & Pickton, Note 1). Although Rescorla and Wagner's model may explain part of the unblocking observed when shock intensity is increased on compound trials, it does not appear that it provides a complete account of unblocking.

This suggests that we should consider more favorably Kamin's (1969) original suggestion that any change in reinforcement sufficient to surprise the subject on compound trials may produce significant conditioning to the added element. There is, however, a certain ambiguity to this suggestion, which may be brought out by considering Kamin's original experiment in which a second, surprising shock is added on compound trials. What is the precise role of this second shock? One possibility is that, because it is expected, it can act as a reinforcer in its own right: In spite of the trace interval separating it from the termination of the compound CS, it is able to support a significant level of conditioning to the newly added element. An alternative possibility, however, is that the second shock does not itself support conditioning, but that, again by virtue of its unexpectedness, its presence somehow ensures that the *first* shock, which would not by itself have acted as an effective reinforcer, is now able to play a normal reinforcing role. Kamin's suggestion that the surprising shock alerts the subject or causes it to engage in retrospective contemplation (Kamin, 1969) is consistent with this latter possibility.

The present results, especially those of Experiment 3, seem to require us to accept the second possibility. In that experiment, the surprising omission of a second shock on compound trials resulted in significant excitatory conditioning to the added element. The omission of shock may support inhibitory conditioning, but it can hardly act as a reinforcing event to produce excitatory conditioning. We must suppose that the role of the surprising event in this case was not itself to reinforce conditioning but to enable conditioning to proceed normally to the otherwise ineffective first shock. This implication is consistent with the analysis of blocking suggested by Mackintosh (1975). On this account, blocking is a consequence of the fact that subjects learn to ignore stimuli that signal no change of consequence. In a typical blocking experiment, conditioning may proceed normally to the new element on the first compound trial, but because it signals no change in reinforcement from that already predicted by the pretrained element, there will be a sharp decline in the value of a stimulus-specific learning-rate parameter, α, associated with the new element. For significant conditioning to occur to the new element, it is only necessary to prevent this decline in α. Provided that it signals some event not already predicted by the pretrained element, whether this be the addition, postponement, or omission of a second shock, the value of α associated with the new element may be maintained at a level sufficient to produce significant conditioning. The surprising event does not itself reinforce that conditioning; it serves to maintain attention to a stimulus that would otherwise be ignored and thus enables the reinforcer actually paired with that stimulus (i.e., the first shock) to have its normal effect. This viewpoint can then explain why Gray and Appignanesi (1973) were able to produce unblocking by presenting a flash of the compound trial stimuli following each compound trial. Such an unpredicted stimulus maintained attention to the added element and thus allowed it to be associated with the reinforcing shock.

The present results have a wider significance than their relevance to theories of blocking. Whether or not one accepts the particular account suggested here, there seems no way of escaping the conclusion that an event of motivational significance such as the addition or omission of the second shock is somehow able to affect the level of conditioning to a particular CS without itself directly reinforcing that conditioning. If we assume that the effect of the surprising event is associative, rather than generally arousing, we must suppose that it is associated with the new element on compound trials, even though it is too far removed in time from the termination of that element to support any change in performance. This implies a rather sharp distinction between the associations that a reinforcer may enter into and the change in the strength of the conditioned response that it may support.

REFERENCE NOTE

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Interference with Autoshaping by Prior Context Conditioning

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Previous research has shown that pretraining with unpredictable conditioned-stimulus–unconditioned-stimulus (CS–US) presentations subsequently retards the acquistion of autoshaping in pigeons. This result has commonly been interpreted as a general transfer-of-training effect; however, more recently, an alternative interpretation based on blocking by contextual stimuli has been suggested by the observation that the retardation effect is context-specific. Experiment 1 replicated the finding that pretraining with unpredictable CS_1–US presentations subsequently blocks the acquisition of autoshaping (where CS_2 and the US are paired) only when pretraining and testing for autoshaping are conducted in the same context. Experiment 2 demonstrated that the context-specific retardation effect can be alleviated by extinguishing the context (presenting the context in the absence of the US). Experiment 3 demonstrated that background contextual cues paired with the US can function as effective blocking stimuli when subsequently presented coextensively with the visual CS in an autoshaping procedure. The results indicate that contextual stimuli are conditionable and can function as effective blocking stimuli. The implications of the data for the interpretation of outcomes in several types of transfer designs were considered.

Brown and Jenkins (1968) reported that hungry pigeons would begin pecking a lighted response key if illumination of the key signaled the forthcoming presentation of grain. The phenomenon is called *autoshaping,* and a number of invstigators have presented evidence that indicates that the acquisition of autoshaped keypecking is governed by the Pavlovian relationship between the lighted key (conditioned stimulus, CS) and the grain (unconditioned stimulus, US) (Gamzu & Williams, 1971, 1973; Wasserman, Franklin, & Hearst, 1974; Williams & Williams, 1969).

Of particular present concern are a number of pretraining manipulations that have been shown to proactively interfere with the acquisition of the keypecking response. These include uncorrelated key-light and food presentations (Gamzu & Williams, 1971, 1973; Mackintosh, 1973; Wasserman, Franklin, & Hearst, 1974), uncorrelated houselight and food presentations (Hall & Honig, 1974), and unsignaled food presentations (Engberg, Hansen, Welker, & Thomas, 1972; Wasserman, 1972).

Several investigators have used cognitive notions like *general attentiveness* (Hall & Honig, 1974), and *learned laziness* (Engberg et al., 1972) in order to account for the retardation of autoshaping observed following these pretraining manipulations. While the particulars of the accounts differ, they share the premise that the retarded acquisition of autoshaping is a general transfer-of-training effect and is a by-product of the associative impairment engendered by pretraining,

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wherein the US is unrelated to stimuli or responses. Thus, learning that the US is unpredictable proactively interferes with the acquisition of autoshaping.

While this interpretation is consistent with the data on autoshaping and is theoretically compatible with interpretations of related Pavlovian phenomena (e.g., Seligman, 1968), an alternative is that the retardation is due to blocking. Using a conditioned suppression paradigm, Kamin (1969) demonstrated that conditioning to a novel CS is retarded if that novel CS is compounded with another CS_X that has been previously conditioned to the US. The phenomenon is called *blocking,* and related work has indicated that the magnitude of the blocking effect is directly related to the amount of CS_X conditioning that preceded the introduction of the novel CS. Therefore, a blocking interpretation can account for differences in the acquisition of keypecking to the autoshaping CS if the key CS is assumed to be compounded with a previously conditioned CS that is more highly conditioned in the groups that are retarded than in the groups that are not.

The blocking interpretation requires the identification of the blocking stimulus, which must be present and conditioned during pretraining and then compounded with the lighted-key CS during autoshaping. The static, background, contextual stimuli of the conditioning environment are present as required. In addition, a growing body of literature indicates that the static situational contextual stimuli are conditioned as a function of their being paired with the US (Dweck & Wagner, 1970; Kremer, 1971; Olding-Smee, 1975; Welker, Tomie, Davitt, & Thomas, 1974), and that contextual stimuli, once conditioned, can subsequently block the acquisition of control over *operant* keypecking in pigeons by a lighted-key CS (Welker et al., 1974). Furthermore, recent theoretical formulations of Pavlovian conditioning suggest that the procedures that produce the retardation of autoshaping are precisely those procedures that favor the development of relatively strong context conditioning (Rescorla & Wagner, 1972).

Recent experiments on autoshaping (Blanchard & Honig, 1976; Tomie, 1976) lend support to a context-blocking analysis. Blanchard and Honig (1976) demonstrated that the speed of acquisition of autoshaping was inversely related to the degree to which the background stimuli present during autoshaping could be assumed to be associated with the US. That is, autoshaping was retarded when administered in the presence of a static former CS+ for food, relative to when administered in the presence of either a former CS− or a novel CS. Their results suggest that autoshaping may be blocked by embedding the CS used in autoshaping within a nonlocalized background CS that has been extensively paired with the US. Further support for a context-blocking analysis was provided by Tomie (1976), who found that the retardation of autoshaping following pretraining with unpredictable US presentations is a context-specific phenomenon. That is, subjects who received pretraining where a tone CS was presented randomly with respect to the food US were subsequently retarded in autoshaping to a visual-key CS, relative to either control subjects whose pretraining was with tone and food paired in a delay conditioning procedure or to control subjects with no pretraining experience, but these differences were eliminated when pretraining and testing were administered in the presence of different contextual stimuli.

The present series of experiments further demonstrates the context specificity of the retardation effects of experience with unpredictable USs. It adds the important observation, anticipatable from the blocking interpretation, that the retardation effect can be extinguished.

Experiment 1

The present experiment was designed to assess the generality of the observation that the retardation of autoshaping following pretraining with unpredictable US presentations is a context-specific phenomenon. Pigeons were tested for the acquisition of autoshaped keypecking to a green-key CS following exposure to a situation wherein the US was presented randomly (uncorrelated) with respect to a pretraining red-key CS (TRC). Half of these subjects were pretrained in the context subsequently used during the test for autoshaping, while half were trained in a different context. The contexts differed in the presence or absence of cardboard lining of the experimental chamber. Retardation effects were evaluated against an original learning control group (OLC) of experimentally naive animals.

Method

Subjects. The subjects were 32 experimentally naive adult pigeons obtained from a local supplier and maintained at 70–75% of their ad-lib weights throughout the experiment. Subjects were housed in individual cages and given free access to grit and water.

Apparatus. Four standard pigeon chambers were used, with associated automatic programming and recording equipment. The chamber measured 35 cm × 35 cm × 30 cm (L × W × H) with a metal intelligence panel on the front containing a 2.9-cm diameter pecking key centered 20 cm above a wire grid floor. Stimuli could be projected onto the response key by Industrial Electronics Engineers in-line display cells equipped with GE 1815 miniature lamps and Kodak Wratten Filters Nos. 99 and 72B, which provided chromatic stimuli with peak wavelength transmission at 555 nm and 606 nm, respectively. The food-hopper aperture was located directly below the pecking key. A houselight was mounted behind a 2.4-cm wide strip of white Plexiglas located above the intelligence panel and provided ambient illumination to the conditioning chamber. Auditory stimuli were provided through a 3-in. (7.62-cm) cone speaker mounted to the right of the food–hopper aperture. Sound sources included a multichannel tone generator which provided a tone of 1,000 Hz, 87 dB (SPL), and a Grason-Stadler Model 901 white-noise generator which provided 92 dB of white noise.

Procedure. Prior to the initiation of the experiment, the 32 subjects were unsystematically divided into four groups of 8 subjects each.

On Day 1, the 16 subjects in the TRC condition were trained to approach and eat from the food hopper. During hopper training the response key was unilluminated. Hopper presentations were made according to a variable-time (VT) 45-sec schedule. Thirty hopper presentations were given on Day 1, and subjects were allowed to feed for 4 sec per hopper presentation. Half of the subjects (identified as in the *no change* condition) were hopper-trained in the chamber in its normal condition. The other half (identified as in the *change* condition) were hopper-trained in a brown corrugated cardboard liner which was built to fit the full inside dimensions of the conditioning chamber. The only apertures in the liner were cut to accommodate the food hopper, response key, and speaker grill. All subjects received their pretraining in the context in which they were hopper-trained.

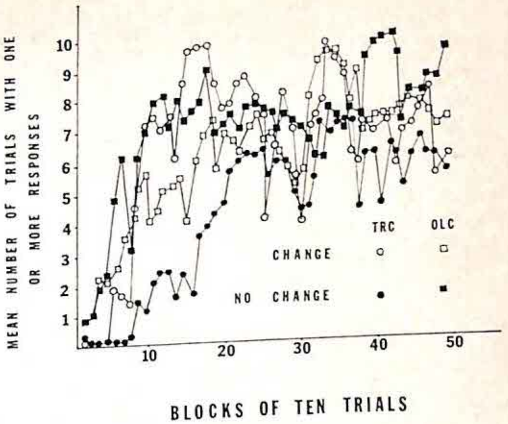

FIGURE 1. Mean number of trials with one or more responses as a function of 10 trial blocks for training red-key (TRC)–*change*, TRC–*no change*, original learning control (OLC)–*change*, and OLC–*no change* groups, Experiment 1.

On Day 2, the 16 subjects in the TRC condition received the first of 10 daily sessions of training, consisting of 60 presentations of 4-sec access to a tray of mixed pigeon grain (US) and a comparable number of 6-sec illuminations of the response key by a 606-nm stimulus (CS). The presentation of the US was arranged to be independent of the presentation of the CS by the use of separate 16-mm film readers to program presentations of the CS and the US.

Following 10 days of training, the TRC subjects were tested for the acquisition of autoshaping in the unlined chamber. Thus, for the eight subjects in the *change* condition, training was conducted in a cardboard environment with cardboard floors, no ambient illumination, and so on, while testing for autoshaping was conducted in a white plastic environment with a gray metal intelligence panel, ambient illumination, wire grid floor, and so on. Autoshaping trials consisted of the illumination of the response key by a light of 555 nm for the 6 sec immediately preceding response-independent 4-sec access to grain. Autoshaping trials were programmed according to the VT 45-sec schedule used in pretraining. Sixty autoshaping trials were given per session. Subjects were run for eight sessions.

The 16 remaining subjects were hopper trained on the day immediately preceding the test for the acquisition of autoshaping and served as original learning controls (OLC). Half of the subjects were hopper-trained in the autoshaping context (*no change*), while half were hopper-trained in the cardboard context (*change*).

Results and Discussion

The mean autoshape acquisition functions for each of the four groups of subjects are presented in Figure 1. As the figure reveals, the subjects in the TRC–*no change* group were retarded in the acquisition of autoshaping relative to subjects in the remaining three groups. The data were entered into a three-way, mixed-design, analysis of variance, with treatment (TRC vs. OLC), context (*change* vs. *no change*) and blocks of trials as factors. The analysis revealed no reliable main effects of treatment or context, but a reliable three-way interaction, $F(47, 1{,}316) = 1.95$, $p < .005$. To isolate the source of the three-way interaction, two separate two-way Treatment × Blocks analyses of variance were performed on the two levels of the context variable. The analyses indicated that under the *no change* condition, there was a significant main effect of treatment, $F(1, 14) = 5.32$, $p < .05$, and a significant Treatment × Blocks interaction, $F(47, 658) = 1.51$, $p < .05$; however, in the *change* condition neither of these effects approached significance ($F < 1$, in both cases). This pattern of results indicates that the effects of the type of pretraining upon the acquisition of autoshaping depended upon the presence or absence of a context change.

Additional evidence that the groups differed in the speed of acquisition of autoshaping was provided by a measure of the number of trials required by each of the subjects to acquire a criterion of responding on five consecutive trials. The mean number of trials to criterion was 258.75, 94.13, 59.38, and 113.13 for groups TRC–*no change*, TRC–*change*, OLC–*no change*, and OLC–*change*, respectively. The data were entered into a two-way analysis of variance which revealed a reliable interaction between type of pretraining and context, $F(1, 28) = 10.82$, $p < .01$. Furthermore, a Duncan Multiple Range test revealed that the TRC–*no change* group differed reliably from each of the other groups ($p < .05$), which did not differ from one another.

The overall pattern of results obtained in this experiment is strikingly similar to that reported by Tomie (1976). The data reported here indicate that pretraining with random CS–US presentations subsequently retards the acquisition of autoshaping to an alternative CS. While this aspect of the results is similar to those reported earlier in autoshaping (Hall & Honig, 1974) and in alternative Pavlovian procedures (Kremer, 1971; Seligman, 1968), this type of retardation effect seems to be restricted to the case where pretraining and testing are conducted in the same environment.

In this experiment, as in related research (Tomie, 1976), the test for the acquisition of autoshaping was conducted in the normal unlined chamber. This confounds the presence or absence of a context change with the locus of the pretraining, and the data, consequently, are subject to interpretation along those lines. Appropriate counterbalancing would require the administration of autoshaping in the unilluminated cardboard lined chamber. Wasserman (1973) has reported that little autoshaping is observed under such circumstances, making the collection of comparable data under appropriately counterbalanced procedures difficult to accomplish.[1] The constraints imposed by this procedural confound require that caution be exercised in interpreting the results of this experiment. The alleviation of this confound by procedural manipulations, which themselves do not directly affect the acquisition of autoshaping, is a matter that must be reserved for later research (see Experiment 3).

EXPERIMENT 2

The blocking interpretation of the results of Experiment 1 rests upon the notion that the static situational stimuli of the conditioning environment are conditioned during pretraining and continue to exert a signaling influence during autoshaping. Experiment 2 sought to evaluate this reasoning in a manner similar to a study reported by Dweck and Wagner (1970). They reasoned that if contextual stimuli can acquire associative strength through a process of Pavlovian conditioning, then the level of conditioning of contextual stimuli should be a function of straightforward conditioning considerations. Therefore, one should be able to weaken or attenuate the control exercised by contextual stimuli by extinguishing the context. One extinguishes a CS by presenting the CS and witholding the presentation of the US. Dweck and Wagner (1970) found that context effects in CER conditioning were reduced when subjects were simply exposed to the context for a number of training sessions in the absence of the US.

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[1] Pilot work at the University of Colorado has indicated that the majority of subjects fail to autoshape in the unilluminated lined chamber.

In Experiment 2, groups of pigeons were subjected to unpredictable US presentations in context and then to autoshaping. For one group, the context was extinguished prior to autoshaping. If the context is conditionable and can then be extinguished, one would expect relatively rapid autoshaping following the extinction of the context relative to appropriate control groups, which did not have the context extinguished prior to autoshaping.

Method

Subjects and apparatus. The subjects were 32 experimentally naive adult pigeons deprived and maintained as those described in Experiment 1. The apparatus was that used in Experiment 1.

Procedures. Prior to the initiation of the experiment, the 32 subjects were unsystematically divided into four groups of 8 subjects each. On Day 1, 24 subjects were trained to approach and eat from the food hopper. During hopper training the response key was unilluminated. Hopper presentations were made according to a VT 45-sec schedule. Thirty hopper presentations were given on Day 1, and subjects were allowed to feed for 4-sec per hopper presentation.

On Day 2, each of these 24 subjects received the first of 10 days of Stage 1 training. Training in Stage 1 consisted of 60 daily presentations of 4-sec access to grain (US). US presentations were programmed according to a VT 45-sec schedule. Stage 1 training was conducted in the normal unlined conditioning chamber described in Experiment 1.

Following this Stage 1 training, the subjects in the extinguish context (EC) condition received two daily 1-hr. sessions of exposure to the conditioning chamber. All US presentations were omitted. The subjects in the extinguish novel (EN) condition also received extinction, except that it was conducted in the cardboard context described in Experiment 1 rather than in the presence of the contextual stimuli present during Stage 1. The subjects in the *hold* condition spent the 2 days in their home cages where they were fed an equivalent of their daily ration of pigeon grain.

Following the 2 days of Stage 2 training, each of the 24 subjects was tested for the acquisition of autoshaping in the Stage 1 context. The procedures used in testing were identical to those described in Experiment 1 except that all subjects received 60 autoshaping trials per session for five rather than eight sessions. The 8 remaining subjects were hopper trained on the day immediately preceding the test for the acquisition of autoshaping and served an an original learning control group (OLC).

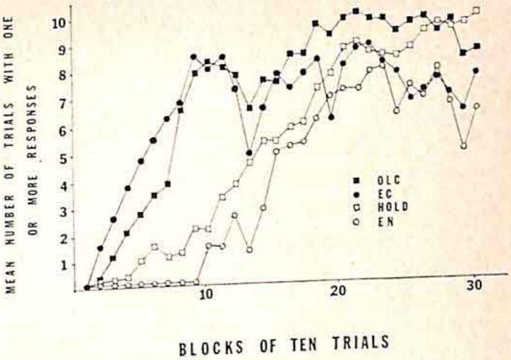

FIGURE 2. Mean number of trials with one or more responses as a function of 10 trial blocks for extinguish context (EC), extinguish novel (EN), *hold,* and original learning control (OLC) groups, Experiment 2.

Results and Disussion

The mean autoshape acquisition functions for each of the four groups of subjects are presented in Figure 2. As the figure reveals, the subjects in the EC and OLC conditions acquired the keypeck response more rapidly than did the subjects in either the EN or *hold* conditions. A two-way analysis of variance indicated a significant groups effect, $F(3, 28) = 5.07$, $p < .01$, and a significant Groups × Blocks interaction, $F(29, 812) = 2.27$, $p < .01$.

In order to determine whether the extinction manipulation had any reliable effect upon the acquisition of keypecking, the EC group was compared in separate analyses to the EN and hold groups. Such planned comparisons yielded reliable Groups × Blocks interactions, indicating that the EC group acquired the autoshape response more rapidly than did either the EN group, $F(29, 406) = 3.56$, $p < .001$, or the *hold* group, $F(29, 406) = 3.52$, $p < .001$.

Additional evidence that the groups differed in the speed of acquisition of autoshaping was provided by an examination of the trials-to-criterion measure. The mean number of trials to criterion was 75.88, 78.00, 143.75, and 184.50 for groups EC, OLC, *hold,* and EN, respectively. A one-way analysis of variance indicated that the differences among the four groups in the number of trials to attain criterion were statistically reliable, $F(3, 28) = 3.30$, $p < .05$. A Duncan Multiple Range test revealed that subjects in the EN condition required more trials to autoshape to criterion than did the subjects in either the EC or OLC groups ($p < .05$). Additional comparisons indicated that the differences between the *hold* group and the EC and OLC groups were only marginally significant, ($.05 < p < .10$). No other comparisons approached statistical significance.

The data indicate that nonreinforced exposure to the autoshaping context had an effect opposite that of reinforced exposure in that it eliminated the retardation normally seen following the latter treatment. Nonreinforced exposure to contextual stimuli other than those of the autoshaping environment did not produce a comparable effect. This pattern of results indicates that the contextual influence seems to be derived from straightforward conditioning considerations. Context control is acquired when the context is paired with the US, and context control is extinguished when the context is not paired with the US. These results are similar to those previously reported by Dweck and Wagner (1970), who used a conditioned suppression paradigm and extended the generality of their results from the aversive to the appetitive realm. The constellation of conclusions supported by the data implicate background contextual stimuli as important associative factors in Pavlovian conditioning.

The data also indicate that the derivation of the contextual influence is not dependent upon the use of a randomly presented CS during the pretraining phase of the experiment. In this experiment, autoshaping was retarded in groups of subjects (EN and *hold*) who were administered pretraining with only random USs in the autoshaping context. The magnitude of this retardation was comparable to that observed in Experiment 1; yet, these subjects had no experience with a pretraining CS presented randomly with respect to the US. The finding that autoshaping is retarded following random US pretraining is consistent with the literature (Engberg et al., 1972; Wasserman, 1972) and consistent with a context-blocking interpretation of the retardation phenomenon.

EXPERIMENT 3

The context-blocking interpretation specifies that the background contextual stimuli of the pretraining environment function as blocking stimuli when compounded with a novel CS. That is, the interference effect is presumed to occur because the contextual stimuli are present and predictive of the US on the occasions in which the visual CS is presented as opposed to other times within the autoshaping session. This was evaluated in Experiment 3 by feeding pigeons in the presence of an auditory background and then providing autoshaping sessions in which that auditory stimulus was coextensive with the visual CS while a novel auditory stimulus was otherwise randomly presented, versus the case in which a novel auditory stimulus was coextensive with the visual CS and the auditory background stimulus was otherwise randomly presented. The use of such a design also allows for the assessment of the effects of the pretraining locus upon autoshaping independently of the effects of changing the context–US relationship.

Method

Subjects and apparatus. The subjects were 24 experimentally naive adult pigeons deprived and maintained as those described in Experiment 1. The apparatus was that used in Experiment 1.

Procedures. Prior to the initiation of the experiment, the 24 subjects were unsystematically divided into three groups of 8 subjects each. On Day 1, 16 subjects were hopper trained in the unlined chamber. On Day 2, the 16 subjects received the first of 10 days of training. Training sessions consisted of 60 daily presentations of 4-sec access to grain (US). US presentations were programmed according to a VT 45-sec schedule. During training, for half of the subjects there was a constant 1000-Hz, 87-dB (SPL) tone present, while for the other half there was a constant 92-dB of white noise.

Following 10 days of training, each of the 16 subjects was tested for the acquisition of autoshaping in the unlined chamber. The test procedures were the same as those used in Experiment 1 with the following modifications. Half of the subjects in each of the above training groups were tested for autoshaping to a 555-nm CS, which was presented at the same time as the auditory CS present during pretraining (context-CS redundant group). Thus, the US was presented immediately following the 6-sec presentation of an audiovisual compound CS. The remaining 8 subjects (novel-CS redundant group) were tested in a similar manner except that the auditory element of the compound CS was novel (for 4 subjects the novel auditory element was white noise, for the remaining subjects the novel auditory element was the tone). For both groups of subjects, the alternative auditory CS was programmed to occur randomly with respect to the US during the autoshaping test. The random auditory CS was 6 sec in duration and was programmed to occur according to the VT 45-sec schedule used in pretraining.

The 8 remaining subjects were hopper trained on the day immediately preceding the test for the acquisition of autoshaping and served as an original learning control group (OLC). The OLC subjects were tested for autoshaping in the same manner as the two experimental groups. Half of the OLC subjects were tested under each condition.

Results and Discussion

There were no differences that approached significance among the counterbalanced conditions within the context-CS redundant, novel-CS redundant, and OLC groups. Therefore, the two conditions within each group were collapsed for purposes of further data analysis. The mean autoshape acquisition functions for each of the three groups of subjects are presented in Figure 3. As the figure reveals, the subjects in the OLC group acquired the keypeck response more rapidly than did subjects in either of the experimental groups. The context-CS redundant group was profoundly retarded in acquiring the autoshape response and exhibited virtually no responding during the entire course of the test. A mixed-design, two-way analysis of variance was performed on the acquisition data and indicated a significant groups effect, $F(2, 21) = 16.27$, $p < .01$, and a significant Groups × Blocks interaction, $F(94, 987) = 4.71$, $p < .01$.

In order to determine whether there were reliable differences attributable to the relationship between the contextual CS and the autoshaping CS, a two-way analysis of variance was performed comparing the performance of the context-CS redundant and novel-CS redundant groups over blocks of trials. The Groups × Trials interaction revealed that the former group was reliably more retarded than the latter, $F(47, 658) = 5.88$, $p < .01$, indicating that the retardation effect is attributable to the prior experience with the contextual CS and the redundant compounding of that CS with the autoshaping CS.

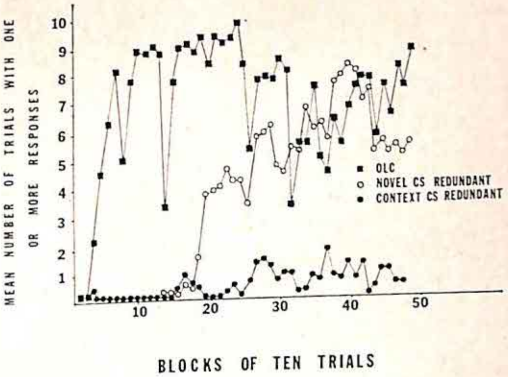

FIGURE 3. Mean number of trials with one or more responses as a function of 10 trial blocks of context-CS redundant, novel-CS redundant, and original learning control (OLC) groups, Experiment 3.

Additional evidence that the groups differed in the speed of acquisition of autoshaping was provided by an examination of the trials-to-criterion measure. The mean number of trials to criterion was 414.50, 208.75, and 69.75 for the context-CS redundant, novel-CS redundant, and OLC groups, respectively. A one-way analysis of variance indicated that the observed differences in the number of trials to criterion was statistically reliable, $F(2, 21) = 11.04$, $p < .01$. A Duncan Multiple Range test revealed that each of the groups differed from each other ($p < .05$).

These results indicate that a background auditory stimulus present during pretraining subsequently blocks the acquisition of autoshaping to a visual CS when the visual and auditory CSs are redundantly compounded in an autoshaping situation. The magnitude of this retardation effect is severely attenuated when the pretraining auditory CS was present but randomly related to the US during testing. The data suggest that the effectiveness of a background context CS as a blocking stimulus is dependent upon its relationship to the visual autoshaping CS and that it is this relationship, rather than the pretraining locus per se, which is responsible for the presence or absence of this type of retardation effect in autoshaping.

The magnitude of the blocking effect obtained in the context-CS redundant group was much more pronounced than the context-blocking effects reported in the previous experiments. The difference may be attributable to a number of factors. Perhaps the enhanced retardation was due to the exceptional effectiveness of auditory stimuli as blocking agents; however, the difficulties encountered by Schwartz (1973), Powell (1973), and Foree and LoLordo (1973, 1975) in developing strong associations between auditory stimuli and food would suggest otherwise.

An alternative explanation attributes the differences in the magnitude of the retardation to the differences in the nature of the relationship between the lighted-key CS and the previously conditioned contextual stimuli. In the procedures utilized in Experiments 1 and 2, the conditioned contextual stimuli provided a background that was compounded, during autoshaping trials, with the autoshaping CS. The contextual stimuli were also present during the intertrial interval, where its presence was nonreinforced (i.e., extinguished). In the present experiment, on the other hand, the contextual stimulus was coextensively compounded with the autoshaping CS and not presented during the intertrial interval. Witholding the presentation of the contextual stimuli during the intertrial interval should eliminate the extinction of the context, which, in turn, should enhance the effectiveness of the context as a blocking agent (c.f., Kamin, 1969; Rescorla & Wagner, 1972).

GENERAL DISCUSSION

The results of this series of experiments provide strong support for a context-blocking interpretation of the unpredictable US retardation effect. Experiment 1 demonstrated that the retardation effect is observed only when pretraining and testing are conducted in the presence of the same contextual stimuli. Experiment 2 demonstrated that the contextual influence is reversible and can be eliminated by providing nonreinforced exposure to the context (i.e., extinguishing the context) prior to the administration of autoshaping. Experiment 3 demonstrated that background contextual stimuli present during pretraining function as effective blocking stimuli when presented coextensively with the visual autoshaping CS, and that the compounding of the context with the autoshaping CS is important to the derivation of the retardation effect. This constellation of results strengthens the context-blocking interpretation by providing direct empirical support for the underlying assumptions of that interpretation.

The data presented here may be accommodated by a number of theoretical formulations of the blocking phenomenon. Kamin (1969) originally accounted for blocking by suggesting that the presentation of the US is reinforcing only to the degree that it is unexpected (i.e., not predicted by the arrangement of alternative stimulus events). The major attributes of Kamin's account have been captured and formalized by Rescorla and Wagner (1972), who proposed that the amount of conditioning accrued to a CS on any given trial is inversely related to the amount of associative strength controlled by the constellation of cues within which the CS is imbedded. Recently, Mackintosh (1975) has proposed an alternative formulation of the blocking phenomenon. Mackintosh suggests that conditioning to the added cue is impaired due to a decrement in the associability of that stimulus with the reinforcer. The associability of the added stimulus is reduced because the added cue signals no change in reinforcement.

While the two positions propose fundamentally different mechanisms for blocking, they differ primarily in the facility with which they accommodate these data. The data are easily handled by the Rescorla-Wagner model of Pavlovian conditioning (Rescorla & Wagner, 1972). The conditioning of background contextual stimuli is an integral part of their theoretical analysis. The extinction of the contextual influence and the incomplete blocking observed in Experiments 1 and 2 follow directly from their formulations. On the other hand, the accommodation of these results by the theory set forth by Mackintosh (1975) is somewhat more strained. The theory makes no direct pro-vision for the conditioning and extinction of background contextual stimuli; furthermore, while Mackintosh's model can readily account for the complete blocking observed in Experiment 3, it is not clear how the model would predict the partial and incomplete blocking observed in the first two experiments.

The possibility exists that the retardation effects reported in these autoshaping experiments are not attributable to associative competition among Pavlovian events, but rather to competition among different instrumental responses. Gamzu (1971) and Gamzu, Williams, and Schwartz (1973) have suggested that the retardation of autoshaping following uncorrelated training is attributable to the development of strong competing behaviors which arise as a direct consequence of intermittent US exposure. They suggest that the topographies of these behaviors tend to be incompatible with the acquisition of autoshaping unless immediately brought under the control of the autoshaping stimulus.

Several lines of evidence oppose such an interpretation. Staddon and Simmelhag (1971) have reported detailed descriptions of the adjunctive behaviors of the food-deprived pigeon in the conditioning chamber. It is difficult to interpret the behaviors they describe as incompatible with keypecking. On the contrary, the patterns of superstitious behavior would tend to bring the pigeon into the immediate vicinity of the pecking key and promote receptor orientation toward that stimulus. Furthermore, extensive but unsystematic observation of the behaviors of the pigeons in these experiments were consistent with the observations reported by Staddon and Simmelhag (1971). The pigeons were observed to orient towards the intelligence panel, with the vast majority of physical movement involving displacement in parallel with that surface. Thus, the assumption that US presentations reinforce skeletal behaviors that are incompatible with the keypeck response seems unfounded.

Furthermore, the additional assumption that instrumentally maintained motor responses necessarily retard the acquisition of autoshaping is highly questionable. LoLordo, McMillan, and Riley (1974) observed that pigeons would readily autoshape while concomitantly maintaining a well-learned treadle-press reponse. Engberg et al., (1972) controlled for an incompatible response interpretation of their retardation effect by training a specific incompatible response (treadle-pressing) for 30 days prior to the initiation of autoshaping. They failed to observe the predicted retardation. In fact, subjects instrumentally reinforced for performing a competing motor response were subsequently observed to autoshape somewhat faster than an OLC group. Tomie (Note 1) has also failed to observe the retardation of autoshaping among a group of pigeons given extensive training (40 sessions) with treadle-pressing for food. In that experiment, the treadle was present during the acquisition of autoshaping and physically isolated from the key. Of further embarrassment to the competing response position, there was no evidence of disruption of baseline rates of treadle-pressing with the introduction of the autoshaping trials or the acquisition of the "incompatible" keypeck response.

In summary, there is little evidence sympathetic with either of the two underlying assumptions of the competing response hypothesis. The available evidence indicates that the behaviors that develop as a direct consequence of intermittent US presentations are not incompatible with the topography of the autoshaping response. Furthermore, experimental evidence indicates that instrumentally maintained competing motor responses do not interfere with the acquisition of autoshaping.

The response competition formulation suggests that instrumental performance factors are responsible for the retardation of autoshaping, while the blocking formulation suggests that Pavlovian acquisition factors are involved. Blanchard and Honig (1976) noted that experimental procedures such as those utilized in these experiments confound the conditions of stimulation under which the keypeck was acquired and the conditions of stimulation under which responding was measured. Thus, in these experiments, it is impossible to separate acquisition effects from effects operative on the performance of the keypeck response, and further analytical experimentation will be required in order to isolate their influences.

The data reported here are sympathetic with a context-blocking hypothesis, and, as such, suggest the reinterpretation of several previously reported demonstrations (c.f., Engberg et al., 1972; Hall & Honig, 1974) of retardation in autoshaping. These demonstrations have been interpreted as instances of general transfer of training effects. For example, Engberg et al., (1972) reported that pigeons were retarded in the acquisition of autoshaping following prolonged experience with noncontingent US presentations. They interpreted their results as an appetitive analogue to the instrumental aversive learned helplessness effect (Maier, Seligman, & Solomon, 1969). That is, they attributed the difficulty that subjects manifest in learning a stimulus-reinforcer contingency (see Gamzu, Williams, & Schwartz, 1973) to the associative interference engendered by prior experience with noncontingent food. The data reported here suggest that the results may be due to the fact that training and testing were conducted in the same environment, and that the differences result from differences in the level of context conditioning at the beginning of the test for autoshaping. Similar US preexposure effects have been reported with other Pavlovian procedures (cf., Mis & Moore, 1973) and are also subject to this interpretation.

Hall and Honig (1974) noted that the speed at which pigeons autoshape varies with the type of pretraining they receive. Pigeons given prior training on a nondifferential multiple VT 1-min. schedule of reinforcement were retarded in the acquisition of autoshaping relative to subjects given prior training on a multiple VT 1-min. extinction schedule of reinforcement. The two components of the multiple schedule were signaled by the wavelength transmitted by the houselights. Testing for the acquisition of autoshaping was conducted in the presence of a novel white houselight in the pretraining environment. Hall and Honig (1974) interpreted this finding as a general transfer-of-training effect attributable to the prior experience with stimulus contingent and noncontingent US presentations. A context-blocking interpretation of their results would suggest that the contextual cues were more highly conditioned in the uncorrelated condition than in the correlated one. Several theoretical accounts of Pavlovian conditioning (Prokasy, 1972; Rescorla & Wagner, 1972) would support this interpretation, and empirical justification for such an interpretation is available (cf., Olding-Smee, 1975).

Similar types of nonspecific contingency effects have been reported in Pavlovian conditioning (Kremer, 1971, Experiment 2; Seligman, 1968), discrete-trial instrumental conditioning (Maier, 1949; Maier & Feldman, 1948), free-operant discrimination learning (Eck, Noel, & Thomas, 1969; Eck & Thomas, 1970; Thomas, Miller, & Svinicki, 1971), and free-operant stimulus generalization (Bresnahan, 1970; Honig, 1969; Switalski, Lyons, & Thomas, 1966; Thomas, Freeman, Svinicki, Burr, & Lyons, 1970; Turner & Mackintosh, 1972). In each of these cases, group differences were observed in the acquisition or assessment of stimulus control following pretraining, which might be expected to generate differences in the amount of contextual conditioning. Therefore, the differences may be attributable to differential blocking or masking (Newman & Benefield, 1968; Thomas, Svinicki, & Svinicki, 1970; Wagner, 1969) of control by contextual stimuli.

The experiments reported here do not comment on the existence of stimulus-related general transfer-of-training effects. The data indicate, however, that many differences that have been interpreted as instances of transfer of training may be analyzed in more specific terms, and that tactics of research should be implemented to control for their possible influences. The applicability of the context-blocking analysis to alternative demonstrations of enhancement or retardation of stimulus control is a matter that certainly warrants further investigation.

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Periodic Decrements in Retrieval of the Memory of Nonreinforcement as Reflected in Resistance to Extinction

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Five independent groups of rats were trained to escape shock in a straight alley on a schedule of partial reinforcement. Differential resistance to extinction as a function of the intertrial interval (ITI) between nonreinforced trials (N) and the following reinforced trials (R) was observed, with resistance to extinction maximal when this interval was 15 min., 12 hr., or 24 hr., and minimal when the interval was 6 hr. or 18 hr. A second experiment, which included continuous reinforcement controls, showed significant partial reinforcement-extinction effects when the N–R ITI was 30 sec, 15 min., 12 hr., or 24 hr., but no partial reinforcement effect was seen in the 6- or 18-hr. groups. These findings suggest that some internal fluctuating aspect of the organism's state mediates retrieval of prior learning or events.

In two recent experiments, Holloway and Wansley (1973a, 1973b) found that rats' retention performance for avoidance tasks is a periodic function of the time elapsed between training and testing. Specifically, retention of both passive and active avoidance learning is maximal shortly after training and at multiples of 12 hr. following training. Retention minima are observed at 6 hr. after training and at multiples of 12 hr. from this 6-hr. posttraining point.

Holloway and Wansley (1973b) suggested two plausible explanations for their finding. First, the internal state of the organism at the time of training, defined by some physiological rhythm, may become a relevant condition for retrieval of the learned response. Shifts away from this state at certain training–testing intervals produce a state-dependent dissociation or stimulus generalization decrement. Alternatively, the training procedure may result in a stress (shock) induced change in the state of the organism resulting either in a series of homeostatic adjustments having a periodic character or in a triggering or resetting of some rhythmic process. This training-induced rhythm could either directly facilitate or interfere with performance or shift the state of the organism away from or closer to that state associated with training.

Two separate questions are contained in these hypotheses. The first concerns the nature of the rhythmic process mediating the multiple retention deficits, whether induced by the training procedure or based on a preexisting physiological rhythm. The second concerns the mechanism of the rhythm's effect on retention preformance, whether it affects performance factors per se or the retrieval of prior learning. The avoidance data of Holloway and Wansley (1973b) argue against a simple type of performance explanation (e.g., rhythmic fluctuations in arousal or activity) in that the pattern of retention deficits was similar for active and passive avoidance tasks that require different response tendencies and was similar across various training or testing times throughout a 24-hr. period. Since a more complex inter-

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TABLE 1
EXPERIMENTAL DESIGN

N–R ITI[a]	Start of acquisition	Extinction
15 min.	0300–0600	0300–0600
	0900–1200	0900–1200
	1500–1800	1500–1800
	2100–2400	2100–2400
6 hr.	0300–0600	2100–2400
	0900–1200	0300–0600
	1500–1800	0900–1200
	2100–2400	1500–1800
12 hr.	0300–0600	1500–1800
	0900–1200	2100–2400
	1500–1800	0300–0600
	2100–2400	0900–1200
18 hr.	0300–0600	0900–1200
	0900–1200	1500–1800
	1500–1800	2100–2400
	2100–2400	0300–0600
24 hr.	0300–0600	0300–0600
	0900–1200	0900–1200
	1500–1800	1500–1800
	2100–2400	2100–2400

[a] Nonreinforced to reinforced intertrial interval.

action between the mediating rhythm and performance factors is still a possible explanation of the phenomenon, the retrieval hypothesis would be buttressed if a similar pattern of retention deficits were found using a task in which the role of performance factors per se is minimal. An empirical answer to the second question, whether the mediating rhythm is induced by the training procedure or is endogenous, requires a behavioral task that allows the separation of the retention interval from the interval between exposure to the training procedure and subsequent testing. The design of the present experiment, derived from Capaldi's (1967) sequential theory of the partial reinforcement extinction effect (PREE), meets both of these requirements.

Capaldi (1967, 1971) has argued that the PREE involves conditioning of the memory of nonreinforcement (S^n) to the instrumental response (R_I). An S^n–R_I association is formed when a nonreinforced trial (N trial) is followed by a reinforced trial (R trial). Resistance to extinction is a function of the strength of the S^n–R_I association. Sequential theory has received considerable support in appetitive studies (e.g., Robbins, 1971), and it has recently been demonstrated that sequential variables account for the effects of partial reinforcement in discrete-trials instrumental escape conditioning (Seybert, Mellgren, Jobe, & Eckert, 1974).

If the state-dependent hypothesis of the multiple retention deficit is correct, the state of the rat at the time of an N trial should become a relevant condition for the retrieval of S^n. As the intertrial interval (ITI) between N trials and following R trials is systematically varied, the availability of S^n, and hence the strength of the S^n–R_I association, should be a cyclic function of this ITI with a period of approximately 12 hr. Behaviorally, groups of rats trained with ITIs between N trials and R trials of 15 min. and multiples of 12 hr. should be more resistant to extinction than rats for which this interval has intermediate values. In addition, such differential resistance to extinction among groups for which the interval between training and testing (extinction) is held constant would argue against the hypothesis that the mediating rhythm is induced by the training procedure. The first experiment, based on this analysis, employed a discrete-trials instrumental escape procedure similar to that used by Seybert, et al., (1974).

EXPERIMENT 1

Method

Subjects. The subjects were 100 male albino rats bred from Sprague-Dawley stock and purchased from Oklahoma Medical Research Foundation, Oklahoma City, Oklahoma. The rats were experimentally naive and were approximately 100 days old at the start of the experiment.

Apparatus. The escape apparatus was a black Plexiglas straight alley with a hinged, clear Plexiglas cover and a grid floor through which footshock could be delivered. Guillotine doors divided the alley into start, run, and goal sections 30 cm, 120 cm and 30 cm long, respectively. The alley was 13 cm wide and 17 cm deep in all sections. Latencies were obtained with an electronic clock, accurate to .01 sec, which was activated by a microswitch at the start-section door and stopped by a photocell unit located 10 cm inside the goal section. A speed measure was obtained by converting the latencies to reciprocals. The microswitch at the start-section door also activated the shock generator (Grayson-Stadler Model #E6070B) which delivered .5-mA current to the start and run section floors. A manually operated switch allowed a current of the same magnitude to be delivered to the goal-section floor. A neon light (two 15-W tubes) was located 24 in. (60.96 cm) above the middle section of the runway.

Procedure. Upon arrival at the laboratory the rats were individually caged and given access to ad lib food and water. Two weeks were allowed for the animals to adapt to the rigid 12-hr. light (0900–2000 hr.) 12-hr. dark (2000–0800 hr.) cycle which was maintained throughout the experiment. On the two days prior to the start of the experiment proper, each rat was individually handled for 6 min. The rats were randomly assigned to five independent groups ($n = 20$/group). In order to control for the possible effects of circadian rhythmic factors on performance, each group was divided into four squads (5 rats/squad) which were trained and extinguished at different points in the light–dark cycle. The training and extinction times for each squad are listed in Table 1.

The schedule of reinforcement used in acquisition was RRRN–RRRRN–RRRRN–R. The five groups differed only in the length of the ITI separating N trials from the following R trials. The values used for this interval were 15 min., 6 hr., 12 hr., 18 hr., and 24 hr. All other ITIs were 30 sec. On R trials, the rat was placed in the start section facing the door, which was raised 5 sec later, activating the shock generator and timer and allowing the rat to traverse the alley. The rat was confined in the nonelectrified goal section for 15 sec. The procedure on N trials differed only in having the goal-section floor electrified. Thus, reinforcement consisted of the termination (in the goal section) of the .5-mA shock encountered in the start and run sections of the alley, while nonreinforcement was the administration of shock in the goal section as well as the start and run sections.

All groups received 20 extinction trials beginning 24 hr. after the last acquisition trial. The ITI used in extinction was 30 sec. On extinction trials, if the rat did not enter the goal section within 60 sec from the time the start-section door was raised, it was placed in the goal section and confined there for 15 sec. When a rat failed to traverse the alley in under 60 sec on three consecutive trials, it was assigned a latency of 60 sec for the remainder of the extinction trials. In all other respects, the extinction procedure was identical to that used for N trials in acquisition.

Results

Acquisition. There were no differences among the groups at the end of acquisition, and no evidence of patterned responding was noted. These conclusions were confirmed by a 5 × 3 (Groups × Trials) analysis of variance performed on the last three trials of acquisition (R, N, R). This analysis indicated a nonsignificant groups effect, $F(4, 95) = 1.28$, $p > .1$, and a significant trials effect, $F(2, 190) = 6.49$, $p < .01$, which reflected increasing speeds over the last three trials for all groups, rather than patterned responding. The Groups × Trials interaction was not significant, $F(8, 190) = 1.36$, $p > .1$.

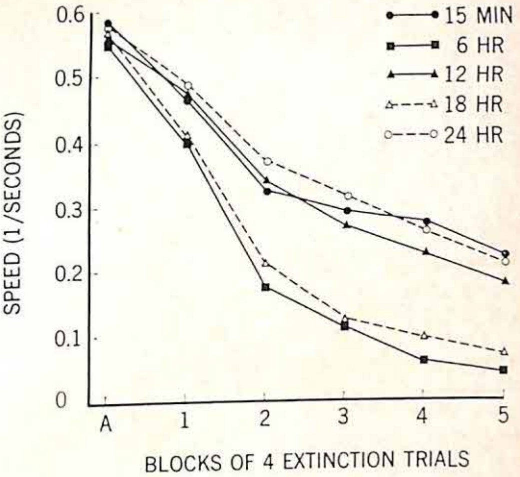

FIGURE 1. Mean running speed during the last acquisition trials (A) and the five blocks of extinction trials (four trials/block), Experiment 1.

Extinction. Figure 1 shows that the 15-min., 12-hr.. and 24-hr. groups (designated by the N–R ITI used in acquisition) displayed greater resistance to extinction than the 6-hr. and 18-hr. groups. A 5 × 5 (Groups × Blocks of Four Extinction Trials) analysis of variance performed on the extinction running speeds indicated a significant effect for groups, $F(4, 95) = 8.47$, $p < .001$, and a significant trials effect, $F(4, 380) = 161.59$, $p < .001$, while the Groups × Trials interaction was marginally significant, $F(16, 380) = 1.47$, $p = .058$, reflecting the divergence of the groups across extinction trials. Comparisons of treatment totals using the Scheffé method for a posteriori comparisons (Winer, 1971) confirmed the equivalence of the 15-min., 12-hr., and 24-hr. groups, largest $F(1, 95) = 2.69$, $p > .1$, and of the 6-hr. and 18-hr. groups, $F(1, 95) = 3.23$, $p > .1$, while the 15-min., 12-hr., and 24-hr. groups were significantly more resistant to extinction than the 6-hr. and 18-hr. groups, $F(1, 95) = 43.33$, $p < .001$.

Figure 2 shows the overall extinction

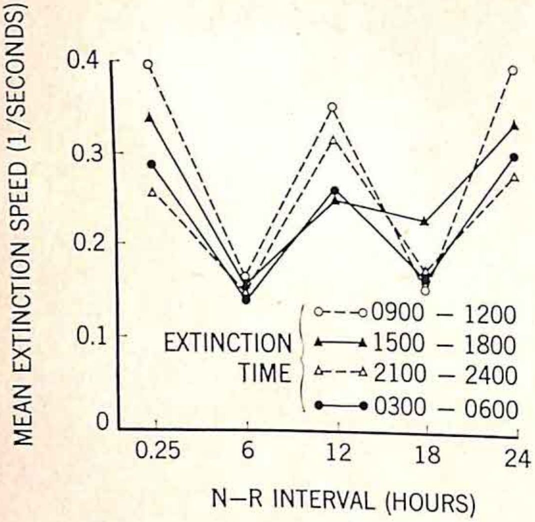

FIGURE 2. Mean extinction scores (average across trials) for animals extinguished at each of the four times during the day, Experiment 1.

scores for all the N–R ITI groups at each extinction time. The pattern of extinction scores across ITI groups is essentially the same. Analysis of variance (N–R ITI Groups × Time of Acquisition) indicated that neither time of acquisition nor the time of acquisition by N–R ITI condition was significant, $F(3,\ 80) = 1.79,\ p > 1$, and $F(12,\ 80) < 1,\ p > .1$, respectively. Similar analysis showed that the time of extinction and the extinction time by N–R ITI interaction also were not significant, $F(3,\ 80) = 1.70,\ p > .1$, and $F(12,\ 80) < 1,\ p > .1$, respectively.

Discussion

The group differences in resistance to extinction observed in this experiment support the hypothesis that some rhythmically varying aspect of the state of the organism present at the time of training may become a relevant factor in the transfer of learning from one trial to the next. The design of the present experiment was derived from Capaldi's sequential theory of the partial reinforcement-extinction effect (PREE). This theory would assume that the PREE observed in instrumental escape tasks is mediated by an association between the memory of a nonrewarded trial (S^n) and the rewarded instrumental response (R_I). The present experiment assumed that the S^n–R_I association would be (a) stronger if the animal experienced the R trial subsequent to an N trial in a state or phase similar to that in which the N trial originally occurred (i.e., 12 or 24 hr. later) and (b) weaker if the R trial occurred in a different state or phase from that present during the N trial (i.e., 6 or 18 hr. later). Basically, the model suggests that some cyclically determined state of the animal modulates the extent to which S^n is retrieved and thus available to enter into association with the response on the subsequent R trial. As resistance to extinction was assumed to be controlled by the strength of the S^n–R_I association we predicted (on the basis of prior training–test interval avoidance data) that animals in the 15-min., 12-hr. or 24-hr. groups would be more resistent to extinction than those in the 6-hr. or 18-hr. groups. The results confirm this prediction.

The results of this experiment further suggest that the pattern of resistance to extinction scores across the N–R interval groups was independent of the time of day during which the acquisition started or during which extinction took place. The design of the experiment (see Table 1) controlled for direct circadian effects by examining four equally spaced periods throughout the 24-hr. day during which acquisition started and during which extinction occurred. The latter facet of the results argue against the hypothesis that the observed differences in extinction are mediated by rhythmic alterations directly interacting with extinction performance. Furthermore, if endogenous rhythms were interacting directly with behavior, differences might have been expected in terminal acquisition speeds, since the experimental treatment (i.e., differing N–R ITIs) occurred throughout training. No such terminal acquisition differences were found.

EXPERIMENT 2

One possible difficulty in interpreting the results of Experiment 1 lies in the absence of continuous reinforcement (CRF) comparisons. The interpretation discussed previously assumed that the differences found in extinction scores in Experiment 1 reflect differences in the magnitude of PREEs rather than some extinction performance phenomenon. However without CRF comparison groups one cannot be sure that such was the case. The demonstration of the PREE is relevant, since it has been shown (Capaldi, Berg, & Sparling, 1971; Seybert et al., 1974) that animals given both partial reinforcement training and extinction trials under massed conditions show a robust PREE, while animals given spaced acquisition and massed extinction trials show a small PREE. Since acquisition trials in Experiment 1 could be described as spaced and extinction trials as massed, it is possible that none of the groups in Experiment 1 would have differed from their CRF controls. In order to assure that the present task parameters indeed produced a PREE and that the extinction data in fact reflect the influence of the N trial experience during acquisition, Experiment 2 partially replicated Experiment 1, with CRF control conditions and a control involving massed acquisition and massed extinction conditions.

Method

The subjects in this experiment were 60 male albino rats obtained from Sasco Co. (a random-bred, Holtzman-derived strain). The apparatus was identical to that used in Experiment 1 except that it was located in a smaller experimental room and the shock generator-scrambler was a Coulbourn Instruments Device (20 independent lines of constant current). The current level used in this experiment was .3 mA. The ad lib maintenance schedule and lighting conditions were identical to that described in Experiment 1. However, the animal housing quarters, the experimental laboratory, and experimenter were different. Animals received handling only when they were weighed, 1–2 days prior to the experiment. The animals were randomly divided into 12 groups, 5 rats per group, representing two sets of conditions: (a) continuous or partially reinforced acquisition trials and (b) six intervals of various duration between certain trials during acquisition. The CRF schedule used in acquisition was RRRR–RRRRR–RRRRR–R, while the partial reinforcement (PRF) schedule used in acquisition was identical to that used in Experiment 1, RRRN–RRRRN–RRRRN–R. The critical intertrial intervals represented by the hyphenated ITIs (the transitions N–R or R–R) were 30 sec, 15 min., 6 hr., 12 hr., 18 hr., or 24 hr. All other ITIs during acquisition and all ITIs during extinction were 30 sec.

The experimental procedures used during acquisition and extinction were identical to those used in Experiment 1 with the exception that the shock level was .3 mA and the shock was distributed across 20 independent shock lines. The 20 extinction trials for each of the 12 groups occurred 24 hr. after the last acquisition trial, between the hours of 9:00 a.m. and noon (the time of day during which extinction occurred was not of consequence in Experiment 1). During extinction, if an animal failed to traverse the alley in under 60 sec from the time the start section door was raised, it was placed in the goal section and confined there for 15 sec. If, on three consecutive trials, the rat continued to fail to traverse the alley in under 60 sec, it was assigned a latency of 60 sec for the remainder of the extinction trials and returned to its home cage.

Results and Discussion

The terminal acquisition speed (1/total latency in sec) and the speed during extinction are presented in Figure 3. There were no apparent differences in terminal acquisition speed among the various ITI groups, the CRF versus PRF conditions or the interaction between ITI and schedule of reinforcement, all $Fs < 2.0$. The overall rate of extinction for groups in Experiment 2 was more rapid than that found in Experiment 1 (see Figure 1), probably because of differences in strain and shock parameters. As can be seen in Figure 3, however, there was a clear difference between PRF and CRF groups when the critical ITI interval was 30 sec, 15 min., 12 hr., or 24 hr., while no differences in extinction of running were obvious when the interval was 6 hr. or 18 hr. Analysis of variance on mean extinction speeds showed significant main effects for ITI, $F(5, 48) = 6.53$, $p < .01$, reinforcement schedule, $F(1, 48) = 48.89$, $p < .01$, and the ITI-schedule interaction, $F(5, 48) = 6.11$, $p < .01$. Simple effect analyses, however, showed that the ITI effect was significant under the PRF condition, $F(5, 48) = 11.73$, $p < .01$, but not under CRF condition, $F < 1.0$. Simple effect comparisons of partial and continuous groups at each interval showed a significant PREE effect at 30 sec, $F(1, 48) = 7.43$, $p < .01$, 15 min., $F(1, 48) = 9.72$, $p < .01$, 12 hr. $F(1, 48) = 16.71$, $p < .01$, and 24 hr. $F(1, 48) = 31.60$, $p < .01$, but no significant differences at the 6-hr.

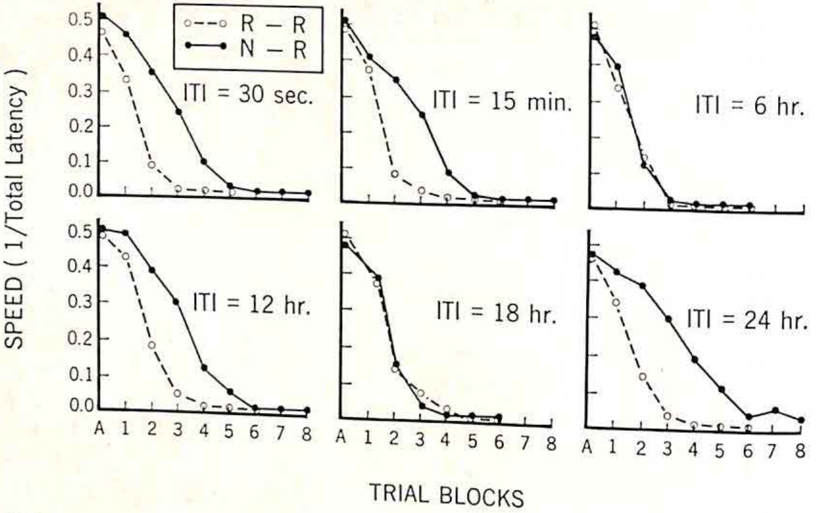

FIGURE 3. Mean running speed during last three acquisition trials (A) and blocks of two extinction trials for continuous (R–R) and partial (N–R) reinforcement groups, Experiment 2.

or 18-hr. interval conditions, both $Fs < 1.0$. An identical pattern of significant and nonsignificant results was obtained by analyzing the number of trials it took the animal to reach the criterion of three consecutive trials with latencies greater than 60 sec.

These results clearly indicate that the pattern of differences in extinction seen in the PRF groups was in fact due to the efficacy of the S^n–R_I association. The 6-hr. or 18-hr. PRF groups behaved in a fashion identical to the 6-hr. and 18-hr. continuous reinforcement groups, a finding consistent with the hypothesis that no S^n–R_I association was conditioned, and hence no increased resistance to extinction was evident in these groups. Furthermore, it should be clear that the 30-sec PRF group was in fact a condition in which the animal received a series of massed training trials ($ITI = 30$ sec) identical to the massed extinction trials ($ITI = 30$ sec). The PREE for the 30-sec group was not significantly different from that of the 15-min., 12-hr., or 24-hr. groups.

GENERAL DISCUSSION

In summary, the findings of the present set of experiments support the view that the state of the organism at the occasion of training or on the occasion of specific trials may be a relevant factor in the retrieval of such experiences during subsequent testing or trials. The present experiments examined resistance to extinction of partially reinforced instrumental escape behavior under conditions in which the interval between nonreinforced and subsequent reinforced acquisition trials was systematically varied. In Capaldi's analysis of this paradigm, if the S^n–R_I association becomes conditioned, a PREE should be evident in extinction. The results in both Experiment 1 and 2 show greater resistance to extinction when the N–R interval was 15 min., 12 hr., or 24 hr. but not evident when the N–R interval was 6 hr. or 18 hr. The results of Experiment 2 show that a PREE indeed was present in the 30-sec, 15-min., 12-hr. and 24-hr. groups but not in the 6-hr. or 18-hr. groups. This set of data support the hypothesis that some endogenous, cyclically determined state of the organism modulates the accessibility of the memory of nonreinforced trials such that the association between the memory of nonreward and the subsequent reinforcing instrumental escape response may or may not become conditioned depending upon the interval between the N and the R trial. The data further suggests that the pattern of differences in extinction scores in the partial reinforcement groups cannot be explained easily by direct influence on extinction performance by endogenous rhythmic factors, as the pattern of extinction across N–R ITIs

(Experiment 1) was similar during extinction at various times throughout the day.

Rejection of the performance hypothesis does not exclude the possibility that the multiple retention deficits observed in the avoidance tasks are due to state-dependent retrieval failures mediated by an *induced* rhythmic process, but this position is also inconsistent with the results reported here. If the relevant state for retrieval of S^n were that induced by exposure to shock on N trials, the same state also should occur when the rat is shocked in the start and run sections of the alley on the following R trials. Periodic deficits in the strength of the S^n–R_I association (and hence, resistance to extinction) would not be expected.

The results of the present experiment represent an important extension of the findings of Holloway and Wansley (1973a, 1973b), demonstrating that the multiple retention deficit has some generality across tasks and across task parameters. A similar retention function has been reported by Hunsicker (1974) for a task requiring suppression of a response having a history of positive reinforcement and by Wansley and Holloway (1975b) for simple retention of a one-trial conditioned approach response for water reinforcement. It also appears that the phenomenon is not dependent on the particular strain of animal used, since the present experiment used rats bred from Sprague-Dawley and Holtzman stocks, while Hunsicker's results were obtained with Long-Evans rats. The role of circadian rhythmic processes in this phenomenon is supported by the recent finding (Wansley & Holloway, 1975a) that lesions of the suprachiasmatic nucleus of the hypothalamus, which abolish circadian fluctuations in activity, eating, drinking, and temperature, also alter the training–testing interval (TTI) retention function for active and passive avoidance behaviors.

In summary, the hypothesis that the cyclic retention function previously reported for one-trial passive avoidance and one-way active avoidance reflects a state-dependent retrieval failure mediated by an endogenous physiological rhythm was supported. The phenomenon has some generality across tasks, task parameters (ITIs vs TTIs), and strain of rat and may be an important consideration for a variety of paradigms involving both retrieval of previous learning and the acquisition of new responses.

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Aversions to Unflavored Water and Cup Drinking Produced by Delayed Sickness

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Rats drank from a cup and were then injected with lithium chloride. The result was an apparent association between the appearance of the cup and toxicosis. In Experiment 1, avoidance of drinking unflavored water from the cup was apparent after two trials in which toxicosis was induced either immediately or ½ hr. after such drinking. Consumption of sucrose solution from the cup prior to toxicosis, however, did not produce a statistically reliable aversion to drinking unflavored water from the cup. In Experiment 2, rats were repeatedly subjected to toxicosis 0.0, .5, 1.5, 4.5, or 18.0 hr. after drinking unflavored water from a cup. A reduction in drinking from the cup was inversely related to the delay of toxicosis, but only the 0.0- and .5-hr.-delay groups exhibited statistically reliable aversions. A later phase excluded the possibility that sensitization alone could account for the cup aversions. Marked aversions to the unflavored water itself were obtained in Experiment 1, but in Experiment 2 any such aversions were so weak that they could not have been responsible for the cup aversions.

Rats develop aversions to smell and taste cues present during feeding if sickness occurs as much as several hours later (e.g., Revusky, 1968; Smith & Roll, 1967; Taukulis, 1974). In contrast, the only attempt to create aversions in rats to other types of feeding cues through delayed sickness was unsuccessful (Rozin, 1969). The apparent limitation of long-delay food aversion learning in rats to taste and smell stimuli deserves reexamination for two reasons. First, in the absence of a lengthy delay between external feeding cues and sickness, rats learn the association (Andrews & Cameron, 1960; Best, Best, & Mickley, 1973; Garcia, Kimeldorf, & Hunt, 1956; Mitchell, Kirschbaum, & Perry, 1975; Overall, Brown, & Logie, 1960; Balagura, Note 1; Riley & Woods, Note 2); in fact, there is even a report that rats can associate a distinctive environment with sickness delayed by as much as 10 min. (Henggeler, Note 3). Second, many species besides the rat appear to associate the appearance of substances and delayed sickness (Gorry & Ober, in press; Johnson, Beaton, & Hall, 1975; Mackay, in press; Wilcoxon, Dragoin, & Kral, 1971); one of these species is the guinea pig (Braveman, 1974, 1975), which is closely related to the rat.

EXPERIMENT 1

Rats were to be trained to associate drinking from a novel steel cup with delayed sickness in the same way they are usually trained to associate drinking in the presence of a novel flavor with delayed sickness. A potential complication in such an experiment is that the rats may associate the flavor of what was consumed from the cup with delayed toxicosis and then associate the appearance of the cup with the flavor. If so, the rats would have a primary aversion to the flavor alone and might avoid drinking from the cup only because they learned that the cup contained a solution with that flavor. Such indirect association of the cup with delayed sickness would not yield any new basic information about learning since there is no doubt that rats can associate flavors and de-

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TABLE 1

EIGHT GROUPS OF EXPERIMENT 1 AND THEIR AVERSION TRAINING: CONTAINER USED, SOLUTION CONSUMED, AND AFTEREFFECT

Groups	Container	Solution	Aftereffect
CupH$_2$O → tox	cup	deionized	immediate toxicosis
CupH$_2$O → 30′ → tox	cup	deionized	delayed toxicosis
CupSuc → tox	cup	sucrose	immediate toxicosis
CupSuc → 30′ → tox	cup	sucrose	delayed toxicosis
BotH$_2$O → tox	bottle	deionized	immediate toxicosis
BotSuc → tox	bottle	sucrose	immediate toxicosis
CupH$_2$O	cup	deionized	no toxicosis
CupSuc	cup	sucrose	no toxicosis

Note. 30′ indicates injection of lithium chloride 30 min. after removal of fluid; the absence of a time designation means that injection was immediate.

layed sickness (Revusky & Garcia, 1970) and little doubt that they can associate external cues and different foodstuffs (Hull, 1933; Leeper, 1935). The question of theoretical importance is whether the appearance of a steel cup can become associated with delayed sickness over and above any association with flavor cues.

Both because of this complication and because of the necessity for various controls, eight groups of rats maintained on deionized water were used in Experiment 1. All of these groups were tested in the same way: by being allowed to drink deionized water from steel cups. They differed during aversion training on three dimensions as outlined in Table 1. These were (a) whether the solution was consumed from the cup or from a bottle with a glass spout, (b) whether deionized water or sucrose solution was consumed, and (c) whether lithium toxicosis was induced immediately after drinking or 30 min. after drinking, or not induced at all.

The first four groups in Table 1 were experimental groups. All of these groups drank from the cup prior to toxicosis so that they had the opportunity to develop cup-toxicosis associations. Technically, the experimental groups were in a 2 × 2 factorial design. One factor was whether deionized water or sucrose solution was consumed prior to toxicosis, and the second factor was the delay of toxicosis. The purpose of the first factor was to assess the aforementioned possibility that any aversion to drinking from the cup is secondary to a flavor aversion. If so, the aversion to drinking from the cup might be expected to vary directly with the primary flavor aversion. This implies that those groups that drank sucrose solution during training would have stronger cup aversions since strong novel flavors like that of sucrose are more readily associable with sickness than flavors as weak and familiar as that of deionized water (Revusky, 1971). But, if the appearance of the cup becomes directly associated with sickness, the rats that drank deionized water from the cup during training ought to exhibit more pronounced inhibition of drinking than those that drank sucrose solution; the sucrose taste, by becoming strongly associated with the sickness, ought to interfere with the association between the cup and the sickness (Rescorla & Wagner, 1972; Revusky, 1971). Furthermore, those rats that drank sucrose solution from the cup prior to sickness during training ought to exhibit a generalization decrement during testing because of the change in the substance consumed from the cup; the rats that drank deionized water from the cup prior to sickness experienced no such change.

Groups BotH$_2$O → tox and CupH$_2$O were controls for Groups CupH$_2$O → tox and CupH$_2$O → 30′ → tox. Group BotH$_2$O → tox allowed an evaluation of sensitization effects in the absence of Cup-toxin pairings, while Group CupH$_2$O allowed evaluation of changes in baseline consumption from the cup in the absence of induced illness. Similarly, Groups BotSuc → tox and CupSuc were controls for Groups CupSuc → tox and CupSuc → 30′ → tox.

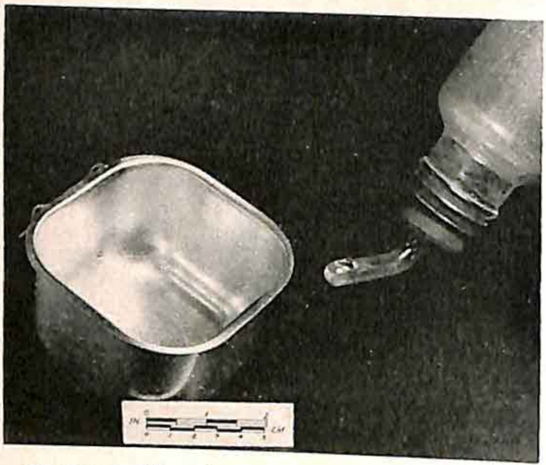

FIGURE 1. The glass spout from which the rats usually drank, and the steel cup from which they drank prior to sickness.

Method

Subjects, apparatus, and materials. The subjects were 123 male Sprague–Dawley rats housed in Hoeltge HB-11A stainless steel cages from which they were never removed except for weighings and injections. They had continual access to dry Purina chow and drank only deionized water from glass spouts, unless otherwise indicated. All subjects were weighed prior to water deprivation and ranged between 193 and 256 g. During the 8 days prior to the experiment, the rats were on a deprivation schedule of 72 hr. without fluid followed by 24 hr. with free access to deionized water in glass spouts. At the termination of the eighth day, as the rats were being deprived, their weights ranged from 233 to 284 g, a substantial increase from the predeprivation level. Group $CupH_2O \rightarrow tox$ had 16 rats. Groups $CupH_2O \rightarrow 30' \rightarrow tox$, $CupSuc \rightarrow tox$, and $CupSuc \rightarrow 30' \rightarrow tox$ had 15 rats each. Groups $BotH_2O \rightarrow tox$ and $BotSuc \rightarrow tox$ had 11 rats each. Group $CupH_2O$ and CupSuc had 20 rats each.

The Hoeltge HB-70A stainless steel cups and the glass spouts used with water bottles are shown in Figure 1. The drinking response elicited by both containers was lapping. The sucrose solution used was 12% (W/V). The lithium injection was 1.0 ml of 2.0% W/V lithium chloride in distilled water administered intraperitoneally with a 26-ga. needle.

Training procedure: Days 1–8. Training trials were administered on Days 3 and 7, while the rats were 64.0–66.5 hr. water deprived. Eight hours after each training trial, the animals were given 24 hr. of free access to deionized water in glass spouts. For half the rats in each of Groups $CupH_2O$ and CupSuc, the second training trial, on Day 7, was omitted; every other rat in the experiment was subjected to two training trials.

Each training trial consisted of 15 min. of access to either deionized water or sucrose solution. The containers used were either the steel cups or the bottles with glass spouts shown in Figure 1. If lithium toxicosis was to be induced for a group, it either was induced immediately after the fluid was removed or 30 min. afterward. The groups and their treatments are outlined in Table 1. The baseline control groups, $CupH_2O$ and CupSuc, simply drank deionized water or sucrose solution, respectively, from the cup without later induction of toxicosis.

Testing phase: Days 9–22. Fluid was unavailable from Day 8 until Day 11, when a regimen of 15 min. per day of access to deionized water began. The container used was a bottle with a glass spout, except on Days 16, 19, and 22, when it was a steel cup.

Results and Discussion

The results of the direct tests of whether the appearance of the cup was associated with toxicosis are not presented until Figure 4. Figures 2 and 3, however, show results o methodological importance. Figure 2 show the mean amount consumed by each grou during each of the two training trials. Eac group subjected to lithium toxicosis afte drinking on the first training trial drar reliably less on the second trial, $ts(9)$ 3.86, $ps < .005$. Groups $CupH_2O$ and Cu Suc, which were the only groups not sul jected to toxicosis after drinking, were al the only groups that did not exhibit a stati tically reliable change. Comparisons betwe pairs of groups are easily interpretable on when both groups drank the same soluti from the same container. Such compariso

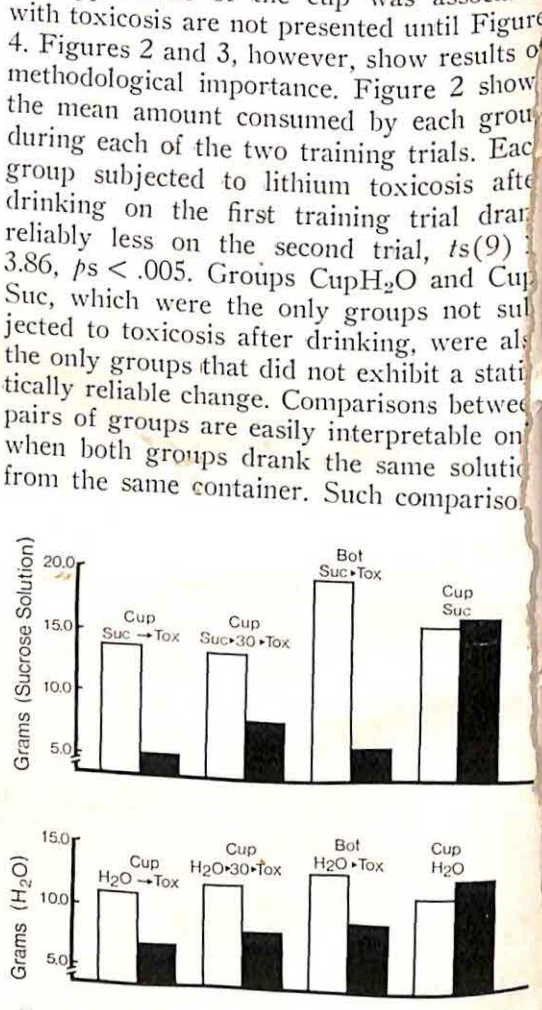

FIGURE 2. Mean amount consumed by each group on the first training trial (white bar) and on the second training trial (black bar), Experiment 1.

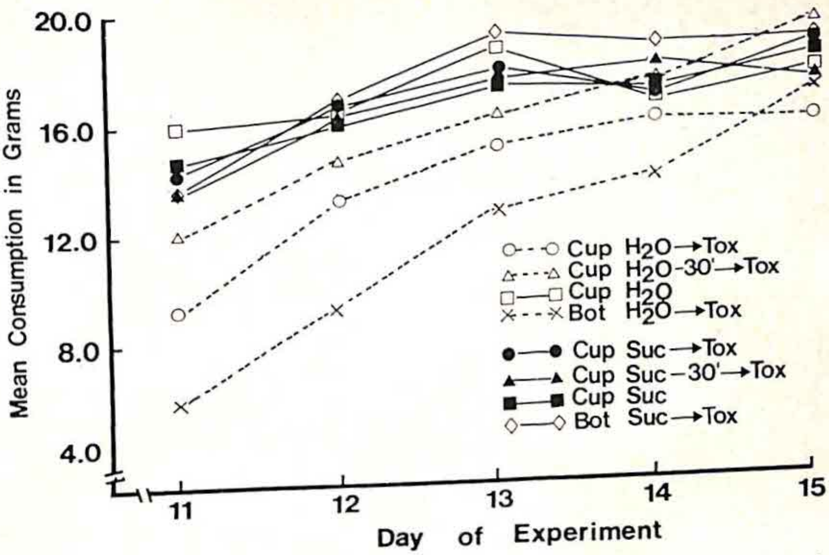

FIGURE 3. Mean amount consumed by each group from the glass spouts during 15 min. of daily access to deionized water initiated 4 days after the last conditioning trial, Experiment 1. (Dotted lines show groups that drank deionized water prior to toxicosis.)

elded marginal evidence for a delay gradi-t since Group CupSuc → tox showed a eater drop in consumption than Group ipSuc → 30′ → tox, $t(28) = 2.46$, $p <$ 5, while the corresponding results for oups CupH$_2$O → tox and CupH$_2$O → 30′ tox were in the same direction but statisally insignificant, $t(29) = 1.56$; combinan of both of these probabilities yields $z =$ 2, $p < .01$.

The aversion to drinking deionized water om the glass spout exhibited on the second aining trial by Group BotH$_2$O → tox in igure 2 was entirely unexpected. This roup had been expected to extinguish any ich aversion during its 24 hr. of free access deionized water from a glass spout during he period between the two training trials. During this 24-hr. period, the mean intake of Group BotH$_2$O → tox was 67.2 g with a range of 49.3–91.4 g; the 24-hr. means for the other groups ranged from 69.5 to 80.9 g. Although the overall analysis of variance was marginally significant, $F(7, 95) = 2.25$, $p < .05$, no differences between groups were reliable at the .05 level of the Newman-Keuls test. Perhaps Group BotH$_2$O → tox extinguished its aversion to drinking deionized water from the glass spouts during the 24-hr. drinking period, and there was spontaneous recovery by the second training trial, which began 64.0–66.5 hr. later. The pattern of results during the 24-hr. drinking period that followed the second training trial was almost identical to that in the earlier 24-hr. drinking period.

The aversions to deionized water are clearly apparent in Figure 3, which shows mean consumption for each group during Days 11–15, when all rats had 15 min. of daily access to deionized water from bottles with glass spouts. An analysis of variance for Day 11 yielded $F(7, 115) = 23.26$, $p < .001$. A Newman-Keuls analysis showed that Groups BotH$_2$O → tox and CupH$_2$O → tox differed significantly from each other and from all other groups at the .01 level. Group CupH$_2$O → 30′ → tox differed from Groups CupH$_2$O and CupSuc at the .05 level. No other differences were reliable at the .05 level according to the Newman–Keuls test. Thus, groups exhibited aversions if, and only if, they consumed deionized water prior to toxicosis during training. The container from which the rats consumed the deionized water was also a factor; if the container used during training was the same as that used during testing, as in Group BotH$_2$O → tox,

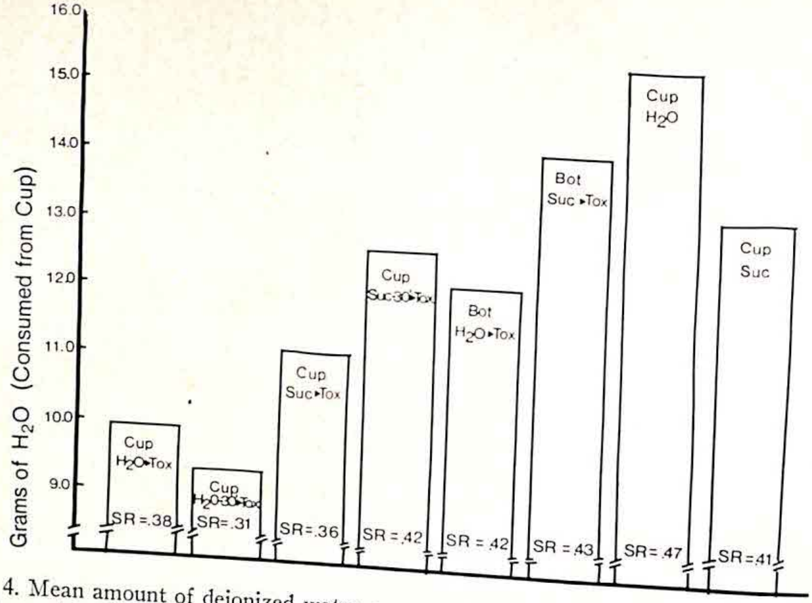

FIGURE 4. Mean amount of deionized water consumed by each group from the steel cup during the first test trial, Experiment 1. (The suppression ratio is indicated near the bottom of each bar.)

the aversion was stronger than if there was a difference in containers, as in Group $CupH_2O \rightarrow$ tox. Similarly, a delay-of-toxicosis gradient was evident since the aversion in Group $CupH_2O \rightarrow 30' \rightarrow$ tox was weaker than in Group $CupH_2O \rightarrow$ tox.

The aversions to deionized water shown in Figure 3 are very surprising. When Garcia and Koelling (1967) obtained aversions to unflavored water, they raised and maintained the rats on saccharin solution to strengthen any aversion by making the unflavored water distinctive during conditioning. We took opposite precautions to weaken the aversion and also interspersed 24-hr. periods of free access to deionized water with the conditioning trials in order to permit extinction. There can be little doubt that experimental conclusions may be in error if they depend on an assumption that rats do not develop aversions to unflavored water (e.g., Cannon, Berman, Baker, & Atkinson, 1975).

One aspect of the data in Figure 3 is relevant to the interpretation of the results of the test for an aversion to the cup, which is presented in Figure 4. Figure 3 gives the impression that the aversions to deionized water were extinguishing over days, and, indeed, by Day 15 there was no reliable difference among groups, $F(7,\ 115) = 1.65$. However, because the amounts shown in Figure 3 constituted the entire fluid intake of the rats, those groups that exhibited aversions were probably developing increasing water deficits, becoming thirstier than the other groups in the course of Days 11–15. Part of the rise in fluid consumption for the groups with aversions to deionized water might then be attributable to an increase in thirst rather than to extinction. The possible differences in thirst level among groups as well as in residual aversions to deionized water must be allowed in interpreting the results of the cup test on Day 16.

Figure 4 shows the mean amount of deionized water consumed from the cup by each group on Day 16. At the bottom of each bar is the suppression ratio obtained by dividing the amount consumed on Day 16 by the total consumed on Days 15 and 16. Suppression ratios adjust drinking scores to the amount consumed on a baseline day, Day 15. In the present circumstance, suppression ratios may be more informative than the uncorrected amounts consumed because of the possible differences among groups discussed in the preceding paragraph. Notice, for example, that Group $BotH_2O \rightarrow$ tox, a control group which is the most divergent group in Figure 3, exhibits no specific aversion to drinking from the cup compared with the spout, as expressed by the suppression ratios, but that it has a slight (albeit insignificant) aversion in terms of the absolute amount consumed relative to the nonpoisoned groups.

An analysis of variance on suppression ratios in Figure 4 yielded $F(7, 115) = 4.69$, $p < .001$. The only group to exhibit a significant aversion relative to each of its controls was Group CupH$_2$O → 30′ → tox: $t(115) = 4.47$, $p < .001$ relative to Group CupH$_2$O and $t(115) = 3.25$, $p < .01$ relative to Group BotH$_2$O → tox. Group CupH$_2$O → tox exhibited a significant aversion relative to Group CupH$_2$O, $t = 2.51$, $p < .02$, but not relative to its other control, Group BotH$_2$O → tox, $t(115) = 1.30$. Since the difference between Group CupH$_2$O → tox and CupH$_2$O → 30′ → tox only approached statistical reliability, $t(115) = 1.95$, and since there was earlier evidence of a delay gradient, it is reasonable to suppose that Group CupH$_2$O → tox had an aversion to drinking from the cup despite the single insignificant comparison with a control.

We stated earlier that if an aversion to the cup's appearance were secondary to a flavor aversion, those groups which consumed sucrose solution from the cup prior to toxicosis ought to exhibit stronger aversions to drinking from the cup than those similar groups that had consumed deionized water from the cup prior to toxicosis. The obtained trend shown in Figure 4 was in the opposite direction. Although both groups that drank deionized water from the cup prior to toxicosis during training exhibited aversions, this was not true of the two similar groups that drank sucrose solution from the cup prior to toxicosis. That is, neither Groups CupSuc → tox nor CupSuc → 30′ → tox drank reliably less than either of the two groups that were appropriate controls for them: BotSuc → tox and CupSuc. Unfortunately, this cannot be considered definitive evidence that consumption of sucrose rather than deionized water interferes with the association of the cup's appearance with later toxicosis. The main reason for doubt is that the comparison of the suppression ratios of Groups CupH$_2$O → tox and CupSuc → tox yielded an insignificant difference contrary to this hypothesis, $t(115) = -.67$. Although the similar comparison between Groups CupH$_2$O → 30′ → tox and CupSuc → 30′ → tox supported this hypothesis, $t(115) = 3.01$, $p < .01$, statistical combination of both of these comparisons did not yield a significant effect, $z = 1.65$, $p = .10$.

Negative results, of course, can often be due to lack of experimental sensitivity. But it is very unlikely that greater sensitivity would have revealed stronger aversions to the cup when sucrose solution instead of deionized water was consumed from the cup prior to toxicosis. The potential artifact in Figure 4 is that the groups that drank deionized water from the cup prior to toxicosis were thirstier due to their aversions than were the groups that drank sucrose solution from the cup (Figure 3). Since thirst reduces the apparent magnitude of aversions (Grote & Brown, 1973), any potential artifact ought to have led to weaker apparent aversions in the deionized water experimental groups than in the sucrose experimental groups. Since the actual trend was in the opposite direction, the evidence is contrary to the hypothesis that flavor aversions comediate container aversions. But this contrary evidence is not definitive. The failure of the sucrose groups to show aversions might conceivably be due to the difference between the sucrose training and the deionized water test conditions; there was no such difference between training and test conditions for the deionized water groups.

The next test of consumption from the cup, on Day 19, revealed no significant difference among groups, $F(97, 115) = 1.09$. Neither these data nor the data from the final test on Day 22 are shown.

EXPERIMENT 2

Experiment 1 showed that associations of the appearance of the cup with toxicosis could be obtained with delays of up to 30 min. Longer delays were not investigated and the evidence for a delay gradient was mixed. In general, the effects shown for the crucial test of Experiment 1 were weak (Figure 4), perhaps partly because of the long delay between the final training trial on Day 7 and the first cup test trial on Day 16, and partly because strong aversions to deionized water interfered with detection of aversions to the cup. Experiment 2 was designed to yield clearer effects.

In the first phase of Experiment 2, rats were subjected to lithium sickness at different delays after drinking tap water from a steel cup; there were nine such trials spaced 4 days apart. On each of the 3 days that intervened between cup-toxicosis trials, the subjects drank tap water from glass spouts without later induction of sickness. An aversion to the cup would become apparent when the rats drank substantially less from the cup than from the glass spouts. It was hoped that the experimental effects obtained would be larger than those of Experiment 1, both because there was an increased number of trials and because, since all trials were now both training and testing trials, there was a reduction of the gap between training and testing. It also was hoped that the increased frequency of 15-min. drinking periods from the glass spouts without toxicosis would attenuate the aversions to unflavored water that had interfered with the interpretation of the results of Experiment 1. Tap water was used instead of the deionized water of Experiment 1 because it was more convenient and because the results in Figure 4 had excluded any reasonable possibility that an aversion to the cup could be mediated through the taste of tap water.

For the second phase of the experiment, the original groups were divided again so as to form four new groups equated for previous history. On five occasions these groups were injected with lithium at different delays after drinking from steel spouts. During the two days between each of these trials, the subjects drank from glass spouts.

Method

Subjects, apparatus, and materials. In unspecified respects, experimental conditions were the same as in Experiment 1. The subjects were 64 rats that had previously been used in a 2-wk. experiment in which they drank coffee and vinegar solutions, sometimes while sedated with pentobarbital. Prior to this earlier experiment, the weight range of the rats was 196–219 g. About 1 wk. prior to the present experiment, it was 243–317 g. Immediately after the present experiment, it was 285–467 g. In addition to the steel cups and glass spouts, steel drinking spouts available from Hoeltge Manufacturing Co. were used. To prevent spillage, these contained steel balls which tended to rattle while the rats drank.

Groups. For Phase 1, the rats were divided into five groups which were to be injected with lithium at different delays after drinking tap water from the steel cups. The delays in hours were 0.0 ($n = 12$), .5 ($n = 12$), 1.5 ($n = 12$), 4.5 ($n = 12$), and 18.0 ($n = 16$). In the second phase of the experiment, each of these groups was divided into four equal subgroups, and each subgroup was subjected to a different delay of toxicosis after drinking from the steel spouts. The delays in hours were 0.0 ($n = 16$), .5 ($n = 16$), 1.5 ($n = 16$), and 18.0 ($n = 16$).

Phase 1. There were nine training trials spaced 4 days apart. On each trial the rat was allowed to drink tap water from a steel cup for 15 min. After an appropriate delay, computed from the time the cup was removed, the rats were injected with 2.0% LiCl solution; the dose was 1.0 ml on the first four occasions and 2.0 ml thereafter. On days when training trials were not scheduled, the rats were allowed 15 min. of access to tap water from a glass spout.

Phase 2. Three days after the last cup trial, there were five trials, spaced 2 days apart, in which the rats drank tap water from the steel spouts for 15 min. and were injected with 2.0 ml of 2.0% LiCl solution afterward. The delay of injection was 0.0, .5, 1.5, or 18.0 hr. depending on the group to which the rats had been reassigned for Phase 2. On the two intervening days, the rats drank tap water from the glass spouts for 15 min. without later induction of sickness.

Results and Discussion

The lower part of Figure 5 shows mean consumption of tap water from the cup on each training day of Phase 1 as a function of the delay of sickness. In addition, consumption of tap water from steel spouts on Day 40 is shown for these same groups; on that day, none of the rats had yet been subjected to sickness after drinking from the spouts. The upper part of Figure 5 shows, for comparison, consumption of tap water from glass spouts on the day preceding each of the trials.

The gist of the results for the steel-cup days in Figure 5 is that the 0.0- and .5-hr.-delay groups exhibited clear aversions to drinking out of the cups; no such clear effect was present on control days when the rats drank from glass spouts. Suppression ratios

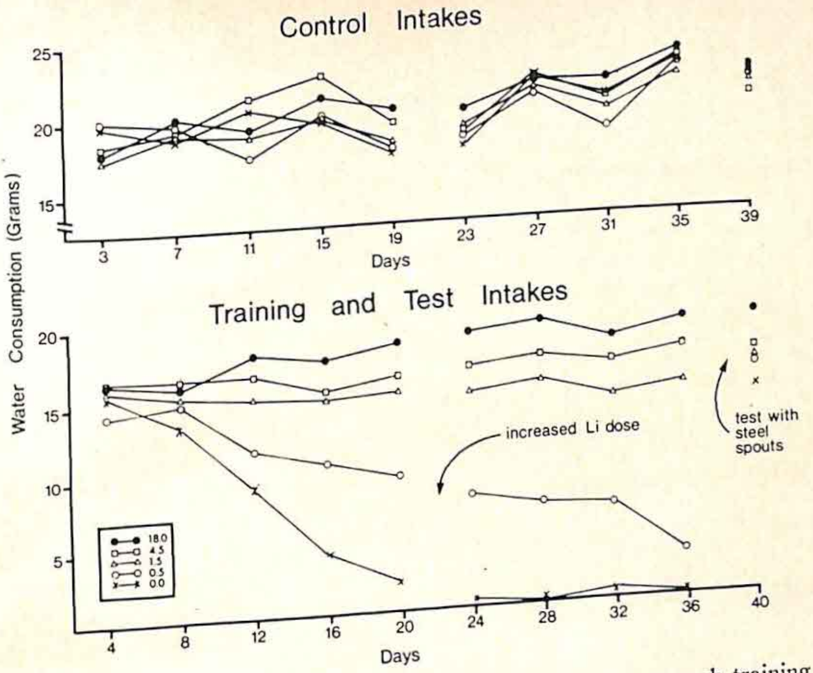

FIGURE 5. Mean amount of tap water consumed from the steel cup on each training trial and the amount consumed on Day 40 from the steel spouts, (lower section); mean consumption by each group from glass spouts on the day prior to each test day, (upper section), Experiment 2. (Datum on the top was obtained the day before the corresponding datum on the bottom.)

were computed for each subject, as in Experiment 1, from the amounts consumed from the steel cups in relationship to the amounts consumed on the preceding day from the glass spouts. A delay-of-sickness trend on the suppression ratios was assessed by the method of orthogonal polynomials; the means of the groups with delays of 0.0, .5, 1.5, 4.5, and 18.0 hr. were weighted −2, −1, 0, +1, and +2, respectively. On Day 8, after only one sickness experience, this delay gradient was significant, $t(59) = 2.93$, $p < .01$, and it became more significant on test days thereafter. Differences within individual pairs of groups were not significant according to the Newman–Keuls test until Day 12, when Group 0.0 drank reliably less than the other groups at the .01 level and Group .5 drank less than Group 18.0 at the .05 level. On all succeeding test days with the cup, Groups 0.0 and .5 were distinct from each other and from each of the other groups at the .01 level according to the Newman-Keuls test. No other group differences were reliable according to this test.

The results in Figure 5 for Day 40, when the rats drank from the steel spouts but had not yet been subjected to toxicosis following such drinking, were obtained to assess the possibility that a sensitization effect rather than associative learning was responsible for the aversions to drinking from the cup. The steel cups were novel relative to the glass spouts, and it might be hypothesized that becoming sick after drinking produces avoidance of any novel container. Such an effect might produce a delay gradient independent of a specific association between the container and the sickness. If so, there would be an equally pronounced delay gradient if a novel container, such as the steel spout, was substituted for the cup. When this was done on Day 40, a reliable and orderly gradient was obtained, $t(59) = 2.89$, $p < .01$, indicating that there may have been a sensitization effect of the type postulated. However, the delay gradient obtained on Day 40 was so much less pronounced than the gradient obtained on Day 36 with cups that it is unreasonable to attribute the latter to sensi-

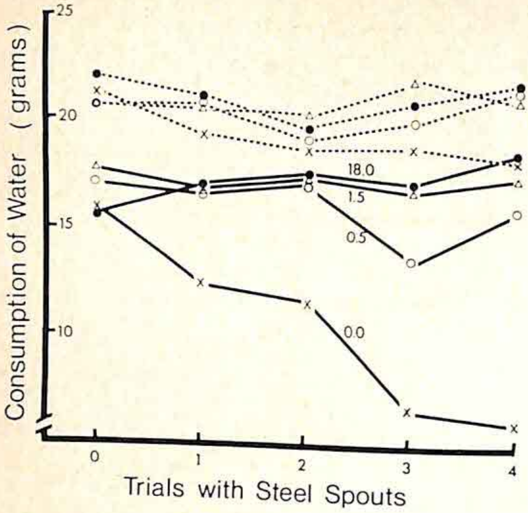

FIGURE 6. Solid lines show mean amounts consumed from steel spouts by each group during each training trial, Experiment 2; dashed lines show the mean amounts consumed from the glass spouts on preceding days.

tization. The results in Figure 6 further support the conclusion that there is a learning effect over and above any sensitization effect: They show that the delay gradient to the steel spouts obtained through an associative learning procedure is far sharper than the gradient obtained on initial testing.

Figure 6 shows consumption for each group on each day that the rats drank from the steel spouts as well as the consumption from glass spouts on the preceding days. Day 0 in Figure 6 is the same as Day 40 in Figure 5, but the data are shown in terms of the groups defined on the basis of the delay of sickness after drinking from the steel spout; the data in Figure 5, of course, are in terms of the delays of sickness after drinking from the cup in Phase 1. For statistical purposes, suppression ratios were used as in the preceding phase, and groups were defined on the basis of the delay of sickness during both Phase 1 and Phase 2 so that any residual effects of Phase 1 could be factored out. Using the resultant error term, we tested for a delay of sickness trend in the data of Figure 6, weighting Groups 0.0, .5, 1.5, and 18.0 as -3, -1, $+1$, and $+3$, respectively. This delay gradient was significant on the first test day (Trial 1 in Figure 6), $t(44) = 2.42$, $p < .02$, and became more significant on later days. According to the Newman-Keuls test, there were no significant differences at the .05 level between pairs of groups until Trials 3 and 4 in Figure 6. Then Group 0.0 drank less than any other group at the .01 level, while none of the other groups exhibited statistically reliable aversions at the .05 level.

There may have been some aversion to tap water in Experiment 2, as indicated in the differences in drinking from the glass spout in the several delay groups. It is obvious, however, from Figures 5 and 6 that any aversions to tap water were far less marked than aversions to the cups or to the steel spouts. The only concern here with aversions to tap water is whether they could be responsible for secondary aversions to the appearance of the cups or the steel spouts. For such a hypothesis to explain the present results, the aversion to drinking from the cup, for instance, would have to be secondary to a far weaker aversion, the aversion to tap water. This seems unreasonable, and so it is concluded that rats can associate directly the appearance of a container and the toxicosis that begins as much as $\frac{1}{2}$ hr. later.

GENERAL DISCUSSION

The main concern in both experiments was whether rats can learn to avoid drinking from a container by associating the appearance of the container with delayed toxicosis. The experimental strategy required that the rats drink something from the container prior to toxicosis. We had assumed that if the substance consumed from the container was familiar deionized water, aversions to its taste would be quite minor, and thus we would easily be able to detect aversions to the appearance of the container. Surprisingly, however, there were pronounced aversions to the deionized water in Experiment 1, and these were not easy to extinguish. The two clearest examples are as follows:

1. On the first conditioning trial Group $BotH_2O \rightarrow$ tox drank deionized water from a glass spout for 15 min. and then were subjected to toxicosis. A few hours later, the glass spouts with deionized water were made available to the rats for 24 hr. In the course of this 24-hr. period, they drank nearly as

much as controls that had not been subjected to toxicosis. Nevertheless, when they were subjected to a second 15-min. trial in which they drank deionized water from the glass spouts, they exhibited a clear aversion to drinking.

2. Rats in Group CupH$_2$O → tox and Group CupH$_2$O → 30′ → tox were subjected to two trials in which they drank deionized water from a steel cup for 15 min. and then were subjected to toxicosis. After each of these trials, they drank deionized water from the glass spouts for 24 hr. without contingent toxicosis. These 24-hr. consumptions were nearly at a normal level. Nevertheless, when the deionized water was subsequently presented in glass spouts for 15 min., they avoided drinking it. Thus, they avoided the deionized water when it was presented in a container that never had been paired with toxicosis. The rats learned this even when the original delay of toxicosis was 30 min., although the effect was more pronounced when there was no delay.

There was also evidence from Experiment 1 that the container from which the deionized water was consumed became associated with toxicosis: (a) When rats drank deionized water from glass spouts, their aversions were more marked when they had previously been subjected to toxicosis after drinking from glass spouts than when they had been subjected to toxicosis after drinking from cups. (b) Rats that had been subjected to toxicosis after drinking deionized water from cups exhibited aversions to the cups over and above their aversions to deionized water when presented in bottles.

The results of Experiment 2 strongly contradicted the mediation hypothesis that rats avoid drinking from containers only because they have learned an aversion to the flavor of the substance in the container. Rats drank tap water from cups and were subjected to toxicosis after different delays: 0.0, .5, 1.5, and 4.5 hr. In Experiment 2 any aversions to drinking tap water from glass spouts were so minor that they can be ignored. Nevertheless, the rats exhibited a pronounced delay-of-toxicosis gradient for drinking from the cups, although only the 0.0- and .5-hr. delays yielded significant aversions. It seemed unreasonable to suppose that these pronounced aversions could have been mediated through the nearly undetectable aversions to tap water itself. A later phase excluded the remote possibility that the aversions to the cup were due to sensitization rather than to associative learning.

We were surprised that rats could associate directly the appearance of a container and toxicosis delayed by 30 min. However, it would be incorrect to claim that this result disproves the original point made by Garcia and Koelling (1966) that rats associate flavor stimuli with sickness far more readily than they associate external stimuli. In the present experiments the container cues were made perfectly predictive of sickness, while the flavor cues were made poorly predictive. The precedents from conventional experiments (Wagner, 1969) are that under such conditions, the flavor cues would be resistant to conditioning. Yet in Experiment 1 one of the weakest flavor cues, that of deionized water, was clearly associated with toxicosis delayed by 30 min. When consumption of deionized water followed by lithium sickness is interspersed with consumption of vinegar solution in the absence of sickness so that the flavor of deionized water has substantial validity, aversions are readily obtained with delays of 4.5 hr. (Revusky & Parker, Note 4). In fact, we are unaware of a single instance in which the present dose of lithium sickness has failed to produce aversions to flavors over delays substantially longer than 30 min. In a series of six experiments, Revusky (1971) allowed rats to drink two substances prior to lithium toxicosis, and the reference-flavored substance always preceded the toxicosis by much more than 30 min.; no failures of conditioning occurred. On the other hand, there has been no successful demonstration of an aversion to external feeding cues in rats with a delay of over 30 min. Thus, the weight of the evidence clearly supports the generalization that rats associate flavor cues with sickness more readily than they associate external cues. The stronger generalization that external cues simply cannot become associated with sickness has been disproven since the work of Garcia, Kimeldorf, and Hunt (1956).

However, the present results do pose a theoretical problem. The dominant theories of food aversion learning presuppose that associations of feeding stimuli with long-delayed sickness occur primarily because of stimulus relevance: Feeding cues become more powerfully associated with sickness than do other cues. Revusky (1971) has argued that if there was no relevance principle to permit association over long delays, extraneous associations of events that occur during the delay with the toxicosis would drown out the long-delay association. The same conclusion can also be derived as a corollary of the similar approach of Rescorla and Wagner (1972), at least when conditioning involves multiple trials. These considerations suggest to us that the feeding response itself mediates the association of external cues with long-delayed toxicosis unless the external cues are made unusually distinctive (Henggeler, Note 3; Wilcoxon, Etscorn, & Czaplicki, Note 5).

Finally, long-delay food aversions have often been explained by the assumption that taste stimuli have temporal properties different from those of the usual external stimuli (Bitterman, 1975; Krane & Wagner, 1975). Such theories cannot account for the present aversions to a cup with a 30-min. delay of toxicosis.

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Effects of Priming Unconditioned Stimulus Representation in Short-Term Memory on Pavlovian Conditioning

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A series of four experiments evaluated eyelid conditioning in the rabbit under circumstances in which the unconditioned stimulus (US) could variously be assumed to be prerepresented (primed) in short-term memory at the time of its presentation in pairing with a conditioned stimulus (CS). In both between- and within-subjects comparisons, less conditioning resulted when CS–US pairings were shortly preceded by US presentations than when not. The decremental effect of the pretrial priming US depended upon the similarity of such US to the subsequent conditioning US included in the CS–US pairing and could be removed by the interpolation of a distractor stimulus between the priming US and the CS–US pair. Finally, a similar decrement in conditioning was observed if a weak priming US was administered prior to the conditioning US but after the CS, that is, within the CS–US interval. The overall pattern of data supports Wagner's thesis that stimuli are rendered generally less effective when they are primed in short-term memory.

The research reported here was directed at evaluating whether or not the effectiveness of a Pavlovian unconditioned stimulus (US) in promoting associative learning is reduced if the US is shortly preceded by another occurrence of the same US. There is reason to anticipate that such might be the case in data from recent Pavlovian investigations (e.g., Kamin, 1969) where the US has been preceded by a conditioned stimulus (CS) previously trained to be associated with the US and in data from habituation studies (e.g., Davis, 1970) that have isolated a short-term refractory-like decrement in unconditioned responding to iterated stimulation. Wagner, (1976) has attributed these, and several related phenomena, to the effects of "priming" of short-term memory (STM), which should also be evidenced in the decreased effectiveness of a Pavlovian US following recent presentation of the same US.

According to Wagner (1976) stimulus presentation will be less effective in instigating processing of that stimulus if the stimulus is already represented (primed) in STM. It may be assumed that a CS, by virtue of prior pairing with a US, will become capable of retrieving into STM a representation of the US stored in long-term memory. In this case, preceding a US by an associated CS should reduce the effectiveness of the US. This has been observed in the so-called blocking effect (Kamin, 1969), in which a US produces less conditioning to a novel CS when the latter stimulus is reinforced in compound with a prior-trained CS, as compared to an untrained CS. It has also been found that a US signaled by an associated CS may elicit a diminished unconditioned response (e.g., Kimble & Ost, 1961; Kimmel, 1966), may produce less interference with the learning about contemporaneous training episodes (Wagner, Rudy, & Whitlow, 1973), and may be less well retained in short-term memory (Terry & Wagner, 1975) than an unsignaled US.

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Wagner (1976) has suggested that the decremental effects of stimulus prerepresentation are indifferent to the manner in which the stimulus is primed into STM. Thus, one would expect a corresponding decrement in effectiveness of a US if representation persisted from a prior presentation of the same stimulus just as from the retrieval action of an associated CS. Evidence of "self-generated priming" (Wagner, 1976) as opposed to "retrieval-generated priming" (Wagner, 1976) has thus far, however, come almost exclusively from studies of the response decrement during habituation. Davis (1970) has presented evidence of a transient decrement in the unconditioned startle response to iterated auditory stimulation, separable from more persistent decrement, while Whitlow (1975) has demonstrated that such short-term decrement in the case of vasomotor habituation is stimulus specific and disrupted by other stimulation that would be assumed to compete for occupancy of STM.

The experiments described herein are most closely related conceptually to the studies of blocking, in which diminished conditioning results from the prior application of an associated CS, and to studies of short-term habituation, in which diminished unconditioned responding results from recent application of the same stimulus. In general, the studies assessed the degree of conditioning that resulted from a conventional Pavlovian training trial, that is, the sequence CS–US, and compared that with the degree of conditioning that resulted from a similar episode in which a priming US, designed to introduce US representation into STM, preceded the final, conditioning US, for example, in the sequence US–CS–US. According to the priming hypothesis, less conditioning to the CS would be expected in this latter condition than in the unprimed condition.

The first experiment evaluated this basic prediction of the priming hypothesis. Subsequent experiments investigated more specific expectations concerning the conditions that should lead to a priming decrement in conditioning. Thus, the second study investigated whether the interpolation of a "distractor" stimulus between the priming US and CS–US presentations would remove the priming US's effects. The third experiment evaluated the presumed importance of the priming US being the same stimulus as that paired with the CS. A final experiment was designed to assess the possibility of a decrement in conditioning when the priming US was presented within the CS–US episode, rather than preceding it. Each experiment also commented on various alternative interpretations of the priming effect. Taken together, the experiments do not necessarily exclude some combination of alternative mechanisms. However, they do show the priming hypothesis to be successful at predicting the outcome of each specific manipulation and, therefore, uniquely sufficient to integrate the findings from the set of studies.

Experiment 1

The hypothesis of self-generated US priming suggests that less conditioning should accrue to a CS paired with a US when the CS–US trial is shortly preceded by a US presentation than when not so preceded. However, there are other reasons why an unsignaled, pretrial US might decrease conditioning under certain circumstances. For example, if training involves a series of trials, the priming US could disrupt the ongoing consolidation (e.g., Grevert & Moore, 1970) or rehearsal (e.g., Wagner et al., 1973) of the preceding CS–US episode. Alternatively, the unsignaled US could lead to a reduction in the validity of the CS as a predictor of US occurrences (e.g., Rescorla, 1967).

In contrast to these latter hypotheses, the priming view suggests that the decremental effect in question should be restricted to those USs occurring shortly after the priming US and not extend to more distantly experienced USs. Neither the retrograde-interference nor the reduction-in-validity hypotheses would anticipate finding less conditioning to the CS involved in a US–CS–US condition than that in a CS–US condition, if the sequence were evaluated in a within-subjects comparison that used an irregular sequence of trials to the different CSs. Konorski, Lubinska, and Miller (1936) conducted a relevant study, in which dogs received, on some trials, a portion of food (the US) 40 sec prior to the pairings of one CS with the US. These episodes were presented in random sequence with trials involving the pairing of other CSs with the US not preceded by a US presentation. Konorski et al. report that conditioned salivation to the CS presented between two USs, as measured on CS-only test trials, was less than to those CSs paired only with a subsequent US. The major reason for caution in interpreting the Konorski et al. findings is that conditioned responses could only be monitored on test trials, when the first US was absent (due to the persisting salivation from the latter US on conditioning trials). As a result, the decrement in conditioned responding that was observed may have reflected generalization decrement from the stimulus conditions present on conditioning trials to those present on test trials.

The first experiment of this report sought to replicate Konorski et al.'s (1936) findings, using a within-subjects eyelid conditioning preparation, with rabbits as subjects. In this preparation, conditioned responses could be monitored both on conditioning trials, when a US alone preceded certain CS–US pairings by several seconds, and on test trials to the CS in the absence of a preceding US.

Method

Subjects. The subjects were 16 male New Zealand White rabbits, weighing between 1.7 and 2.8 kg at the start of training. Each was individually housed and provided with ad libitum food and water.

Apparatus. The subjects were trained in one of eight identical 66 × 48 × 48-cm isolation chambers. Each chamber was completely lined with aluminum foil to provide a homogeneous visual surround and was illuminated dimly by a 15-W neon light mounted on the rear wall. The chambers were maintained at a 67-dB (A) sound level (General Radio meter #1551-C) by the presentation of white noise through a 12.7-cm loudspeaker also mounted on the rear wall. Within the chamber, the rabbit was further confined in a 51 × 18 × 14-cm Plexiglas restraining box, through which only its head protruded.

Three conditioned stimuli were used: (a) 3,500-Hz tone presented through the same loudspeaker as the white noise, increasing the sound level 10 dB as measured at the locus of the animal's head; (b) 15/sec flashing-light CS generated by a Knight KG-323 strobe lamp located behind the subject so as to diffusely reflect from the walls of the chamber; (c) vibrotactual stimulus presented by means of a hand-massager (Valmour Model 880) mounted on the floor of the restraining box so as to maintain firm contact with the rabbit's chest. The unconditioned stimulus was a train of 100/sec 5-mA square-wave shock pulses produced by an Argonaut LRA-046 constant-current generator and delivered through stainless-steel electrodes (Sklar surgical wire, #32 gauge) sutured in the skin about the orbit of the rabbit's right eye. One electrode was implanted approximately 5 mm below the extreme nasal extent of the eye and the other approximately 5 mm above the extreme lateral extent.

The CSs were uniformly 1,100 msec in duration, and the US was always 100 msec in duration. Whenever a CS was reinforced, the US overlapped, and terminated with, the CS.

Closures of the subject's right eyelid were monitored by a microtorque rotary potentiometer taped to the animal's head and communicating with the lid by a length of thread hooked to a small permanent suture loop. Movements of the eyelid turned a counterweighted wheel affixed to the axle of the potentiometer and were graphically recorded on a Beckman Dynograph, with an effective gain of 5.

Procedure. On the day prior to the start of training, the subjects were habituated to confinement in the restraining boxes outside of the isolation chambers for 2 hr. During this time, their heads were shaved and the electrodes implanted.

All subjects then received 8 days of acquisition training with two CSs. On the training trials, one CS was simply consistently reinforced by the presentation of a US (i.e., the sequence of stimuli being CS–US). This CS, the conditioning episodes of which were preceded by *no* priming stimuli, will thus be referred to as $\mathrm{CS_N}$. The second CS was similarly reinforced by a US but was also preceded 4 sec earlier by a US (i.e., the sequence of stimuli being US–CS–US). This CS, the conditioning episodes of which were preceded by a priming-US presentation, will thus be referred to as $\mathrm{CS_{US}}$. Eight trials with $\mathrm{CS_N}$ and eight with $\mathrm{CS_{US}}$ were presented in each session, in a sequence that balanced first-order transitional probabilities. Two replications of eight animals were run, the first of which used the tone and vibrotactual stimuli in counterbalanced designations as $\mathrm{CS_N}$ and $\mathrm{CS_{US}}$, and the second similarly used the tone and flashing light.

In addition to the above-described training trials, nonreinforced test trials to $\mathrm{CS_N}$ and $\mathrm{CS_{US}}$ were also presented in which neither stimulus was preceded or followed by a US. In the first replication, three test trials with each CS were presented in a block following the conditioning trials of each session, two sequences of $\mathrm{CS_N}$ and $\mathrm{CS_{US}}$ trials equating order of presentation across blocks of two days. For the second replication, two test trials with each CS were embedded within each daily sequence of training trials: On odd-numbered days Trials 7 and 12 involved $\mathrm{CS_N}$, and Trials 9 and 15 involved $\mathrm{CS_{US}}$, this order being reversed on alternate days.

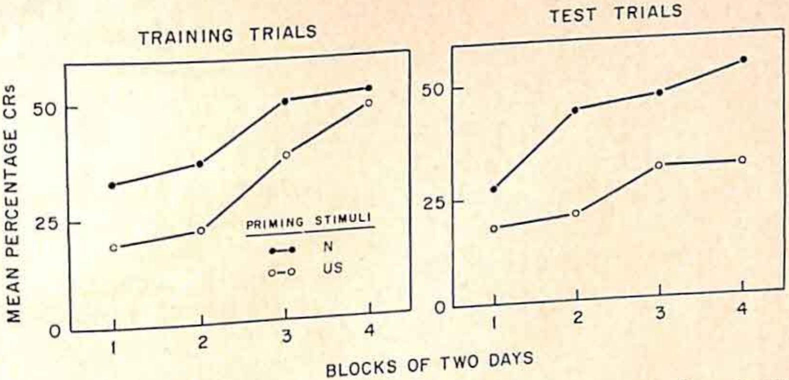

FIGURE 1. Mean percentage conditioned responses to two CSs trained in Experiment 1 plotted over 2-day blocks of trials, as a function of the priming stimuli presented prior to the CS–US trials: no priming stimuli (N), or a priming US. (Response frequency is shown in the left panel on training trials and in the right panel on test trials.)

The mean intertrial interval between all training and test trials was 4 min. Thus, each session lasted approximately 90 min.

Scoring. A conditioned response (CR) was recorded if a pen deflection from the pre-CS baseline of at least 1 mm (.2-mm eyelid movement), in the direction of eyelid closure, occurred in the interval from 140 to 1,000 msec following onset of either CS.[1] Inspection of the polygraph records of CS_{US} trials showed that after the termination of the priming US there was a return of the eyelid to the pre-US resting level within 1 to 2 sec, well before the onset of the CS. Conditioned responses to CS_{US} measured in relationship to the pre-CS baseline would therefore almost invariably occur relative to the same baseline as would responses to CS_N, that is, the pretrial resting position of the eyelid. Further description and analysis of the response typography may be found in Terry (1976). The amplitude of the unconditioned responses (URs) to the USs was not measured, as the degree of amplification used to monitor the presence of CRs precluded obtaining a record of the total extent of the UR.

Results and Discussion

Inspection of the data revealed little difference between the two replications of the experiment in the levels of responding to either CS_N or CS_{US} and between the vibrotactual and light stimuli (hereafter considered together as the vibrotactual/light cue) used in the separate replications. The two replications were therefore pooled, and in Figure 1 are plotted the overall mean percentages of CRs to CS_N and CS_{US} across successive 2-day blocks of trials. The left panel shows conditioned responding on the training trials, when reinforced CS_N presentations were not preceded by priming stimuli (N), but reinforced CS_{US} presentations were preceded by priming stimuli (US). The right panel shows conditioned responding to the same CSs on test trials, when the US did not occur immediately preceding or following either CS.

It may be noted first in Figure 1 that on each of the several blocks of training trials the CS paired with an unprimed US (i.e., CS_N) evoked a higher frequency of conditioned responses than did the CS paired with a primed US (i.e., CS_{US}). That the presence of a pretrial US was not simply causing a transient suppression in responding to the subsequent CS_{US} may be seen by inspection of the test trial data. Here the frequency of conditioned responding to CS_{US} was again less than to CS_N, on each of the several blocks of trials.

A mixed-factor analysis of variance of the mean percentage CRs to CS_N and CS_{US} was conducted to evaluate the above observations. Within-subjects factors were the comparisons of the two CSs (CS_N vs. CS_{US}), the different cues (tone vs. vibrotactual/light), the trial types (training vs. test), and the four blocks of days. Between-subjects factors were the two replications of the experiment and the two groups of subjects formed by the assignment of the tone cue as either CS_N or CS_{US} (with the vibrotactual/light cue as the other CS). The first, and most important finding of this analysis was a reliable difference between the overall conditioned responding to the cues designated as CS_N and CS_{US}, $F(1, 12) = 6.46$, $p < .05$. This difference in responding to the two CSs was relatively stable over trials and test conditions: The advantage of CS_N over CS_{US} did not interact reliably with blocks of days, $F < 1$, with trial type, $F(1, 12) = 3.39$, $.05 < p < .10$, nor with the combination of Trial Type × Blocks of Days, $F(3, 36) = 1.46$, $p > .10$.

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[1] The amplitude criterion in Experiment 1 was less than that used in Experiments 2–4 and was instituted in order to include smaller amplitude responses to the tone stimulus not otherwise employed as a CS in the experimental treatments. Scoring the data with the more conservative criterion of Experiments 2–4 revealed the same pattern of results as those reported, the only differences being a lower overall level of conditioned responding and a greater apparent difference in percentage responding to the light and vibrotactual CSs versus the tone CS.

There was no overall difference in the total conditioned responding to CS_N and CS_{US} between the two groups of subjects formed by the designation of the tone as either CS_N or CS_{US}, $F < 1$. There was, however a substantial overall difference in conditioned responding to the different cues used variously as CS_N and CS_{US}, $F(1, 12) = 15.65$, $p < .01$, with the vibrotactual and light stimuli eliciting comparable levels of responding in the two replications (overall means = 48.6% and 45.7%, respectively), which were each higher than that elicited by the tone stimulus (overall mean, across both replications, =24.5%). As all subjects received both the vibrotactual/light and tone stimuli, the presence of a cue effect would tend to increase the CS_N–CS_{US} difference within some subjects while decreasing this difference within other subjects. Therefore, an additional statistical assessment of the effect of the priming US was conducted, using a between-subjects comparison of each cue when used as CS_N and as CS_{US}. These comparisons showed that the vibrotactual/light cue was reliably more responded to when trained as CS_N than when trained as CS_{US} (overall means = 51.6% and 43.0%, respectively), $t(12) = 2.34$, $p < .05$, while the tone cue was similarly reliably more responded to when trained as CS_N than when trained as CS_{US} (means = 34.4% and 14.0%), $t(12) = 5.72$, $p < .01$.

Finally, as is apparent in Figure 1, the analysis revealed a reliable increase in responding to the CSs across the four blocks of days, $F(3, 36) = 6.74$, $p < .01$. All remaining main effects and interactions of the analysis were nonsignificant.

On the final days of training there appeared to be a higher frequency of responding to CS_{US}, but not to CS_N, on the training trials as compared to the test trials. This observation must be cautiously evaluated in view of the nonsignificant overall effect of trial type and the nonsignificant CSs × Trial Type × Blocks interaction. However, a post hoc comparison of training- versus test-trial responding to CS_{US} on the fourth block of days was reliable, $t(15) = 2.70$, $p < .05$. There are several reasons why such superiority of responding to CS_{US} on training trials over test trials might be observed. For example, there could have been some generalization decrement on test trials due to the change in the stimulus conditions from those on the training trials. Alternatively, the priming US could have become a discriminative stimulus for the conditioning US that occurred on the training trials but not on the test trials. A third hypothesis is that on training trials the priming US had a performance-facilitating effect, as US representation in STM may summate with a similar representation activated by the CS leading to increased likelihood of a CR (Pfautz & Wagner, 1976). The performance on the last block of trials suggests that certain of these processes may have played some role in determining responding, but in general the same processes are minimally indicated by the remaining overall pattern of the data.

Experiment 1 thus appears to demonstrate that the presence of a US preceding the CS–US trials reduces conditioning below that obtained when the CS–US pairing is not preceded by a US. These results are consistent with the assumption that the priming of the US in STM via an earlier US presentation can lead to a reduction in the conditionability of the US.

These findings cannot be readily explained by acknowledging that a pretrial US should depreciate CS validity (Rescorla, 1967), or disrupt consolidation (Grevert & Moore, 1970) or rehearsal (Wagner et al., 1973) of the preceding trial. The present within-subjects design, with a counterbalanced sequence of trials, insured that such effects would equally affect conditioning to both CSs.

However, there are still some potential alternatives to the priming hypothesis for the decrement in conditioning to CS_{US}. Discussion of these alternatives will be presented later when considering the results of those experiments that provide data relevant to their evaluation.

EXPERIMENT 2

The priming hypothesis presumes that the deficit in conditioning to CS_{US} in Experiment 1 was due to the active representation of the priming US in STM at the time of occurrence of the conditioning US. According to this view, any manipulation that would remove the priming representation from STM would also remove the decrement in conditionability of a subsequent US. It is frequently assumed (e.g., Wagner et al., 1973) that the retention of stimulus information in STM can be disrupted by the occurrence of other stimuli that themselves command occupancy of STM. It should therefore be possible to reduce the priming effect by following the priming US with the presentation of a novel stimulus that would displace the earlier US's representation from STM. Following analogous reasoning, Whitlow (1975) found that placing a "distractor" stimulus between two presentations of the same stimulus removed the short-term habituation of responding otherwise seen to the second occurrence of the stimulus.

Experiment 2 sought to determine whether or not a distractor stimulus interpolated between the priming US and the subsequent CS–US pair would reduce the magnitude of the priming decrement as observed in Experiment 1. A comparison condition was also included to determine whether the distractor alone had any facilitating of interfering effects on conditioning.

Method

Except where noted below, the apparatus and procedures were the same as in the first experiment.

Thirty-two naive rabbits, similar to those in Experiment 1 were assigned to four, equal-sized groups. All subjects received conditioning trials with a single CS, a 1,100-msec flashing light, consistently reinforced by a 5-mA, 100-msec US. For subjects in three of the groups, one or both of two pretrial stimuli were also used: a priming US, identical to the US used to reinforce the CS, and a distractor stimulus.

The selection of the distractor stimulus followed from the assumption that a novel, dynamic stimulus would more successfully displace the pretrial US representation from STM than would a repeated unitary stimulus. Each distractor instance consisted of the sequential presentation of several different brief stimuli, with the same stimulus sequence never being repeated. Specifically, the 1-sec distractor was composed of five 200-msec intervals of stimulation, each interval involving one of three different stimuli, a train of 30/sec clicks produced by a relay mounted in a metal sounding box located on the floor of the isolation chamber 17 cm to the left of the subject's head, the previously described tone, and the previously described vibrotactual stimulus. Of the possible sequences (for example, the sequence of click [C], tone [T], and vibrotactual [V] stimuli in one distractor stimulus could be C–T–V–T–C or T–V–V–C–V, etc.) a pool of 30 were drawn at random, except for the constraints that no 2 sequences could have more than three of the same stimuli in the same positions, and that each of the three different stimuli were approximately equally often represented in each of the five positions across the 30 sequences. All subjects scheduled to receive distractors then experienced the same order of exposure to the sequences over appropriate trials, without repetition.

The conditioning experiences received by the four groups of eight subjects were identical except for the stimuli presented prior to the CS–US pairings on the training trials. Group N had no pretrial stimuli scheduled, and simply received a CS–US pairing on every trial. Group US received a priming-US presentation 7 sec prior to the onset of the CS–US trial (i.e., the sequence US–CS–US). These two groups correspond to the CS_N and CS_{US} conditions, respectively, of Experiment 1, but in a between-groups design. A third group, US + D, received a priming US 7 sec before the CS–US trial and also a distractor stimulus (D) beginning 2 sec after the priming US and 5 sec before the CS–US pairing (i.e., the sequence US–D–CS–US). A fourth group, Group D, received only the distractor stimulus, with onset 5 sec before the onset of the CS–US trial (i.e., the sequence D–CS–US).

All subjects were given 12 total CS–US conditioning trials, at an average intertrial interval of 5 min., on each of three daily sessions. On 10 of the trials, designated as training trials, the different groups of subjects received the pretrial stimuli described above. On the remaining two test trials, the 6th and 11th trials of each session, the CS–US pairings were presented in the absence of any pretrial stimuli, to provide an assessment of the amount

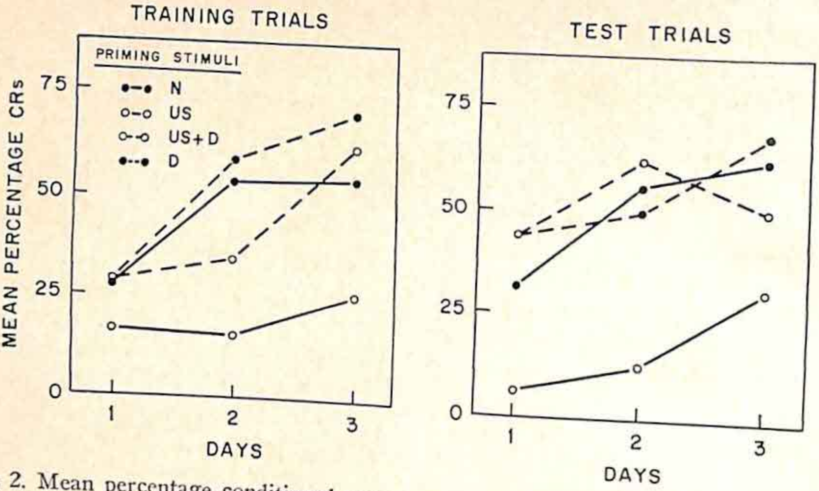

FIGURE 2. Mean percentage conditioned responses to the CS of Experiment 2 plotted over 3 days of training, as a function of the priming stimuli presented prior to the CS-US training episodes: no priming stimuli (N); a priming US; a priming US followed by a distractor (D) stimulus; or only a distractor. (Response frequency is shown in the left panel on the training trials occurring proximal to the test trials of each session and in the right panel on the test trials.)

of responding evoked by the CS free of any transient effects of the pretrial stimuli.

A CR was defined as a 2-mm or larger pen deflection (.4-mm eyelid movement) in the direction of eyelid closure. As in Experiment 1, the records of subjects were inspected for changes in the eyelid baseline following the pretrial stimulus occurrences. There was found to be a rapid return of the eyelid to its resting level following a pretrial US and following a distractor stimulus, when responses to the latter occurred. Thus, the presence of pretrial stimuli did not preclude consistent scoring of CRs to the CS in all groups in terms of eyelid deflection from the pre-CS baseline level. (See Terry, 1976).

Results and Discussion

Inspection of the mean percentages of responding for the several groups showed a slightly higher frequency of conditioned responses to the CS on the daily test trials than the mean level of responding across all of the daily training trials. This difference, however, was due to the fact that the test trials were arranged to occur, on the average, later in the daily sequence of trials and thus reflect a greater degree of prior conditioning. Therefore, a total of four training trials, those immediately preceding and following each of the two test trials, were selected from each session for all further analyses, so that the average position in the session of the analyzed training trials equaled that of the test trials.

Figure 2 displays the mean percentages of CRs to the CS over the three days of acquisition training, plotted separately for the four groups. The left panel shows conditioned responding on the selected training trials and the right panel shows conditioned responding on the test trials.

The first question of interest is whether a priming-US alone reduced conditioning on the just-following CS–US trials, as was observed in Experiment 1. As can be seen from the overall levels of responding in Figure 2, those subjects receiving only the priming-US prior to the CS–US occasions (i.e., Group US) showed less responding to the CS than those subjects not receiving a priming stimulus (i.e., Group N). The important new finding in the present study is that the presentation of a distractor stimulus between the priming US and the CS–US occasions (i.e., Group US + D) removed the deficit in conditioned responding found with a pretrial US and produced a level of responding comparable to that obtained in the absence of pretrial stimuli. Thus, even though Groups US and US + D both received priming USs, only the former group, which had the US-to-CS–US interval free of extraneous stimulation, showed a decrement in conditioned responding to the CS. This apparent removal of the deficit obtained with a priming US was not due to the distractor being a general facilitator of conditioning, as shown by the conditioned responding of those subjects receiving only the distractor prior to the CS–US presentations (i.e., Group D). This group's responding was not notably different from that of the group receiving no pretrial stimuli. In brief, the major difference among the several groups was the depressed responding to the CS in Group US, relative to Groups N, US + D, and D, on both training and test trials.

These observations were supported by a mixed-factor analysis of variance of the mean percentage CRs to the CS, which used the four experimental groups as a between-subjects factor, and the trial type (training vs. test) and three days of conditioning as within-subjects factors. There was found to be a marginally reliable overall difference among the groups, $F(3,\ 28) = 2.94$, $p \approx .055$, with no significant interactions of groups with either trial type or days or with the combination of these factors, (all ps $> .10$). Subsequent Newman-Keuls comparisons showed that subjects in Group US responded reliably less than those in Groups N, US + D, or D (all ps $< .05$), while the latter three groups did not differ significantly among themselves.

The overall mean percentages of responding did not differ between training and test trials, $F < 1$, and trial type did not significantly interact with the other factors (all ps $> .10$). There was a reliable increase in conditioned responding over days, $F(2,\ 56) = 11.43$, $p < .01$, but days did not interact with any other factor (ps $> .10$).

In addition to monitoring CRs to the CS, eyelid responses during the distractor in Groups US + D and D were also scored on the selected training trials of the first and third days represented in Figure 2. The criteria for scoring a response were the same as those for recording CRs to the CS, except that short-latency responses (i.e., less than 140 msec) were also counted. For comparison, responses to the CS on these trials were also rescored to include the same duration, although this only minimally increased response frequency. Responses during the distractor stimulus were found to occur on about 36% of the trials examined. Of particular interest is the fact that the percentage of trials on which the distractor evoked a response did not significantly change between Days 1 and 3 (31.75% and 40.0%, respectively), $F(1,\ 14) = 2.32$, $p > .10$, while responding to the CS greatly increased (28.1% and 67.2%), $F(1,\ 14) = 14.63$, $p < .01$. Closer inspection indicated that the responses to the distractor appeared to have a different typography than the CRs to the CS. Thus, the mean latency of response onset to the distractor was longer than that to the CS, 537 msec versus 364 msec, $t(15) = 2.60$, $p < .05$. Also, responses to the CS were consistently of longer duration than those made to the distractor, even when comparing responses to the two stimuli that were matched for onset latency (e.g., of those responses beginning between 180 and 360 msec after stimulus onset, 56% of the responses to the CS and 29% of the responses to the distractor continued for 540 msec or longer). It is thus possible that the responses to the distractor were not CRs, or at least not of the same class of CRs as the responses to the CS, but rather were orienting and gross body-movement responses evoked by the novel stimulus sequences. It may also be noted that these differences between CS- and distractor-evoked responses were found in similar degrees in both Groups US + D and D. Thus, the immediately preceding US in Group US + D did not noticeably affect responding to the distractor.

The results of the present experiment support the hypothesis that the decremental effect of a pretrial US on conditioning acts via an STM mechanism, such that the displacement of the US prerepresentation from STM by a novel stimulus reinstates the conditionability of the subsequent US.

The present data also comment on certain alternative interpretations for the effects of the pretrial US on conditioning in Experiments 1 and 2. For example, it could be argued that the pretrial US "overshadowed" conditioning to the CS via some form of associative competition (e.g., Pavlov, 1927; Rescorla & Wagner, 1972), or otherwise interfered with either the recognition or processing of the subsequent CS. However, neither an overshadowing hypothesis nor a "masking" hypothesis would have readily anticipated the reduction in the priming US's effect which occurred when a distractor stimulus was presented between the pretrial US and CS–US pairing. Another interpretation would suggest that both a response-suppressing aftereffect of the prior US (present only on training trials) and a generalization decrement (present only on test trials) occurred to produce less responding to the CS in the priming conditions. However, this notion would then predict at least comparable generalization decrement on test trials for those subjects trained with both a pretrial US and a distractor and for those subjects trained with the pretrial US only. Yet, on the test trials, subjects in Group US + D responded more than did the subjects in Group US.

EXPERIMENT 3

One of the most powerful predictions of Wagner's (1976) account of the conditioning deficit, observed in Experiments 1 and 2, is that the priming effect should be relatively specific to occasions in which the priming US is the same as the subsequent conditioning US: The effect should vary with the degree to which the priming US enters a representation in STM that corresponds to the later stimulation. Such stimulus-specific priming effects have been observed by Whitlow (1975) in the previously mentioned studies of habituation, and by Pfautz and Wagner (1976). The latter authors found a reduction in the conditioned responding to a well-trained CS when it was shortly preceded by the same CS but not when preceded by a different CS or no CS.

Experiment 3 was designed to evaluate the specificity of the US priming effect on conditioning. It involved varying the locus of administration of the pretrial US relative to the locus of administration of the subsequent conditioning US; that is, the pretrial US was delivered to either the same eye or a different eye than the conditioning US. According to Wagner (1976), a more potent US priming effect should be produced when the US representation in STM closely corresponds to the subsequent US than when it does not.

Method

Except where noted below, the apparatus and procedures were the same as in the previous experiments.

In the present experiment, subjects were trained in only four of the isolation chambers. In two of the chambers, the potentiometers were adapted to record movements of the left eyelid, while in the other two, movements of the right eyelid were monitored as in the previous experiments. All of the chambers were modified so that a US could be applied to either the area of the right or left eye.

The subjects were 16 experimentally naive rabbits, prepared as usual on the day prior to the start of training, except that (a) half of the subjects had loops sutured to the left eyelid, rather than to the right, and (b) all subjects had electrodes implanted about the orbits of both the right and left eyes.

On those trials designated as training trials, all subjects received conditioning episodes consisting of a priming US, followed by a CS–US pairing. The USs were 100-msec, 5-mA shocks, administered variously to either the area of the right or left eyes according to experimental plan. The CS was a 1,100-msec flashing light stimulus. For half of the subjects, the priming US was given to the *same* eye (US_s) as the conditioning US. Four of these subjects received the priming and conditioning USs of each episode to the right eye, and four subjects received both USs to the left eye. For the remaining subjects, the priming US was given to the different eye (US_d) from the conditioning US. Thus, four of these subjects received the priming US to the right eye and the conditioning US to the left eye, with the reverse designation being used for the last four subjects. The priming US-to-CS interval was 7 sec.

Subjects received 5 daily 1-hr. conditioning sessions, each containing 10 CS–US training trials, on which the appropriate pretrial USs were administered. In addition, two CS–US test trials (the 6th and 11th trials of each session) were presented without any pretrial stimuli. The mean intertrial interval was 5 min.

Conditioned responses were monitored as in Experiment 2 from the eye receiving the conditioning US, this being the right eye for subjects run in two of the isolation chambers and the left eye for subjects run in the other two chambers. Half of the subjects trained in each of the four chambers received the priming USs to the same eye as the conditioning USs and half to the different eye. Thus, the locus of the conditioning US was confounded with chambers, while the US_s versus US_d conditions were balanced within chambers.

Results and Discussion

As in Experiment 2, the training trial data that were subjected to analyses were taken from those trials that immediately preceded and followed the two test trials in each

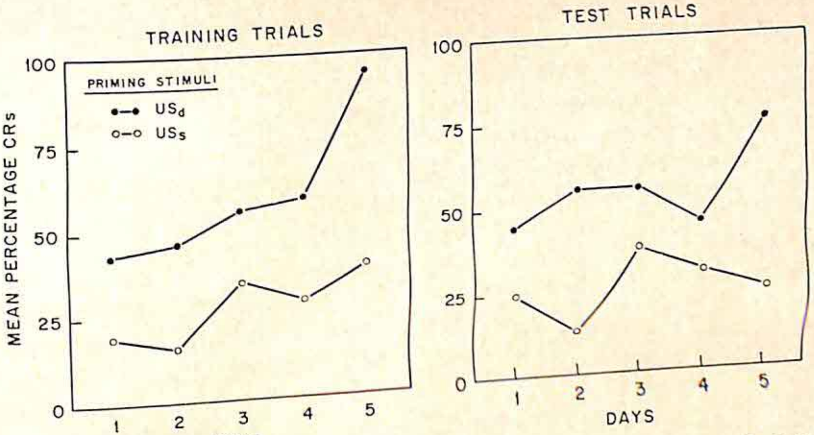

FIGURE 3. Mean percentage conditioned responses to the CS of Experiment 3 plotted over 5 days of training, as a function of receiving, prior to each CS–US training episode, a priming US to the same eye (US_s) as the conditioning US, or to a different eye (US_d). (Response frequency is shown in the left panel on the training trials occurring proximal to the test trials of each session and in the right panel on the test trials.)

session. In Figure 3 are plotted the mean percentages of CRs over each of the 5 days of acquisition on the selected training trials (left panel) and on the test trials (right panel). The two group functions represented in Figure 3 are differentiated on the basis of whether the priming US was administered to the same eye as the conditioning US (Group US_s) or to the different eye than the conditioning US (Group US_d). As can be seen, there was more conditioned responding to the CS by those subjects receiving the priming US and conditioning US at different loci than by subjects receiving both USs to the same locus. This was apparent on the training trials when the pretrial USs were present, and on the test trials when the pretrial USs were absent.

A mixed-factor analysis of variance was conducted on the mean percentage CRs over the 5 days of acquisition training. The between-subjects factors used were the two groups of subjects, defined by the locus of the priming US relative to the conditioning US (US_s vs. US_d), and the four different conditioning chambers (as inspection of the data revealed different overall response levels among the subjects trained in the different chambers). The within-subjects factors used were the type of trials (training vs. test) and the 5 days of conditioning. The results of the analysis showed that the difference between Groups US_s and US_d was highly reliable, $F(1, 8) = 17.85$, $p < .01$. There was no significant difference between training and test-trial responding and no interaction of trial type with the US_s versus US_d groups, Fs < 1. While there was a significant increase in responding over days, $F(4, 32) = 4.01$, $p < .05$, there was no reliable interaction of the frequency of responding between the two groups of subjects over days, $F(4, 32) = 1.80$, $p > .10$.

There was a significant difference in the overall mean percentages of CRs among the subjects trained in the different chambers, $F(3, 8) = 19.13$, $p < .01$. However, this did not affect the primary findings of the study, as there was no reliable difference in the US_s versus US_d effect among the different chambers, $F < 1$.

The results of this experiment argue that there is a high degree of specificity of the decremental effect of pretrial USs on conditioning during subsequent CS–US episodes. Thus, a CS embedded within a US–CS–US episode showed less evidence of conditioning, both on training and test trials, when the two surrounding USs were identical than when they differed only in their locus of administration. This finding is consistent with the notion that the representation of a US in STM interferes with the processing of a subsequent occurrence of that same US.

The present observations argue even more strongly than those of Experiment 2 against an overshadowing or a masking interpretation of the pretrial US's effects. According to these hypotheses, the priming US should have had equivalent effects on the subsequent CS in both Groups US$_s$ and US$_d$, as the experimental treatment of administering the conditioning US to the same or opposite eye did not occur until after the CS. However, conditioning was still found to be less in the *same* than in the *different* condition.

EXPERIMENT 4

The first three experiments have provided results consistent with the hypothesis that a self-generatively primed US is reduced in its effectiveness in promoting conditioning with a concurrently presented CS. In these studies, priming was effected through presentation of a US prior to the CS–US episode. However, the priming hypothesis does not require that the priming US precede the CS–US pairing but rather only that it precede the final, conditioning US. Thus, a priming-like effect should be producible using an arrangement of stimuli different from the US–CS–US sequence but that still preserves the priming, US–US relationship. The particular sequence of stimuli used in Experiment 4 placed a priming US *within* the CS–US conditioning trial, rather than preceding it. One group of subjects received pairings of a brief CS followed by an intense US. A second group received these same CS–US pairings but in addition consistently received a less intense value of the US interpolated between the CS and the intense US. The expectation was that the interpolated US, by priming a US representation into STM, would reduce the conditioning produced by the subsequent intense US. And, if the interpolated US, because of its weak intensity, did not itself produce substantial conditioning to the CS, there would be less conditioning observed to the CS paired with two USs than to the CS paired with one US.

Certain other procedural changes were made in the present study. First, the CS was terminated prior to the usual time of occurrence of the priming and conditioning USs. It could thus be assumed that the priming US could not act by interfering with CS reception. Second, all subjects received pretraining with two other CSs. One stimulus, CS$^+$, was paired with weak and intense USs, both alone and in sequential compounds, with the various CS–US intervals to be used in the later phase of the experiment. This pretraining was calculated to produce subjects that might be expected to condition to the subsequently presented target CS as a function of the effective intensity of the US paired with it but that were otherwise equated for general experience with certain necessary differences of the comparison conditions. The latter include such features as receiving one versus two US occurrences or one versus two US intensities within a single trial or having a regularly shorter or longer CS–US interval. A particular desideratum was that the occurrence of a weak US would not be an accurate overall predictor of the occurrence of an intense US for any subject. The second CS, CS$^-$, was always nonreinforced and occurred on trials interspersed among those with CS$^+$. The CS$^-$ training was intended to reduce generalized response tendencies from CS$^+$ to the subsequently trained target CS.

Method

Except where noted below, the apparatus and procedures were the same as in Experiments 1 and 2.

A total of 32 experimentally naive rabbits received an initial phase of discrimination training with a reinforced CS (CS$^+$) and a nonreinforced CS (CS$^-$). For half the subjects a 150-msec tone was CS$^+$ and a 150-msec flashing light was CS$^-$, this designation being reversed for the remaining subjects. Within CS$^+$ trials, a 100-msec shock US was scheduled to occur at 1,200 and 1,800 msec following CS onset and on each occasion took one of three values: no US, 1-mA US, or 5-mA US. This resulted in nine possible US arrangements of which only one was not used, the case in which no-US occurred both 1,200 and 1,800 msec following CS$^+$. CS$^+$ was thus paired with the following arrangements of 0-, 1-, or 5-mA USs, occurring after the 1,200 and 1,800-msec delays, respectively: 5–5; 5-1; 5–0; 1–5; 1–1; 1–0; 0–5; 0–1.

Sixteen CS$^+$ trials occurred in the daily sessions, with each of the above mentioned US arrangements being used twice, along with 16 nonreinforced CS$^-$ trials presented in a sequence that balanced first order transitions between CS$^+$ and CS$^-$ trials. Half of the animals were given 26 days of discrimination training; and, as here appeared to be little additional benefit toward discriminative performance from the last several days of training, the other half of the subjects were given only 23 days. The mean intertrial interval was 3 min., for a total session duration of about 96 min.

Of the 32 animals started, only 24 were used in the second phase of the experiment. One subject died, and 7 others were dropped for failure to reach a discrimination criterion of at least 25% difference in the mean percentage responding to CS^+ and CS^- over the last 5 days of training. The mean difference for the latter, eliminated subjects was approximately 3%. Two groups of 12 subjects, matched for their performance to the discriminative CSs, were formed from the remaining animals. The groups were equated for amount of prior discrimination training and for designations of tone and flashing light as CS^+ (with the other cue as CS^-).

The two groups then received the experimental phase of training, consisting of the pairings of a new, vibrotactual CS and the US. For one group, no priming stimuli were presented on these trials (Group N), while for the second group, a priming US was presented (Group US). Specifically, for Group N, a 150-msec CS (henceforth referred to as CS_N in reference to this group) was followed after 1,800 msec by an intense (5-mA) US. For Group US, the same CS (referred to as CS_{US} in reference to this group) was followed after 1,200 msec by a weak (1-mA) US, and after 1,800 msec by an intense US. Thus, CS_N was reinforced by an unprimed, intense US, while CS_{US} had a less intense priming US interpolated between the CS and intense US.

Five trials per session were given with CS_N and CS_{US} in the separate groups, distributed among the 32 regular discrimination trials, with the restriction that CS_N or CS_{US} always followed a CS^- trial, separated by the usual intertrial interval. The first four CS_N and CS_{US} trials in each session were reinforced by the US events appropriate to the two groups, while the fifth trial was a nonreinforced trial involving neither US. Three sessions of experimental training were given.

Conditioned responses were scored using criteria identical to those used in Experiments 2 and 3, except that responses were counted as CRs if they occurred in the interval from 140 to 1,200 msec following onset of any of the CSs.

Results and Discussion

The acquisition of the discrimination between CS^+ and CS^- for those groups of subjects that eventually received experimental training is shown in the left panel of Figure 4. The mean percentages of CRs to CS^+ and CS^- are plotted across blocks of 2 days for the first 22 days of training received by all subjects. As can be seen for these groups of subjects, the magnitude of the discrimination was substantial: The difference between CS^+ and CS^- averaged about 56% over Days 20 to 22, with little notable difference between those subjects designated as Group N and Group US.

Responding to these same discriminative stimuli on the *first* experimental training session is shown in the right panel of Figure 4, which plots the proportion of subjects making CRs on single CS^+ and CS^- trials occurring proximal to each of the CS_N and CS_{US} trials of Groups N and US, respectively. As can be seen, the two different groups maintained approximately equivalent amounts of responding to each discriminative CS over the course of the session.

Also shown in the right panel of Figure 4 are the data of primary interest, the proportion of subjects making a CR on each trial of Day 1 to CS_N, which was paired with an intense US in the absence of priming stimuli (N), and to CS_{US}, which was paired with an intense US primed by a weak US presentation (US). As with CS^+ and CS^-, the reported CRs are those occurring within 1,200 msec of CS onset on each trial. Although both Groups N and US started with comparable levels of responding to the new CS (the Trial 1 difference is not reliable, $z = .47$), by Trial 5 there was more responding to that CS reinforced by one US (i.e., CS_N) than to the CS reinforced by two USs (i.e., CS_{US}). On this last trial, 9 of 12 Group N subjects responded to CS_N, but only 2 of 12 Group US subjects responded to CS_{US}, $z = 2.46$, $p < .02$.

The observation of differential acquisition of CRs to CS_N and CS_{US} was supported by the results of an analysis of variance of the proportion of CRs across blocks of Trials 1 and 2 versus 4 and 5. The analysis revealed a marginally significant difference between the two groups, $F(1, 22) = 4.20$, $.05 < p < .10$. However, there was also a reliable CS × Trial Blocks interaction, $F(1, 22) = 5.67$, $p < .05$. Subsequent comparisons revealed that there was no difference in responding to CS_N and CS_{US} on the block of Trials 1 and 2, $t(22) = .86$, but that CS_N was responded to reliably more frequently than CS_{US} on the block of Trials 4 and 5, $t(22) = 2.86$, $p < .01$. There was also an increase in conditioned responding to CS_N from Trials 1 and 2, to 4 and 5, $t(22) = 2.89$, $p < .01$, while there was no reliable change in responding to CS_{US}, $t(22) = .48$.

Of some interest is the fact that by the end of the session Group N subjects responded

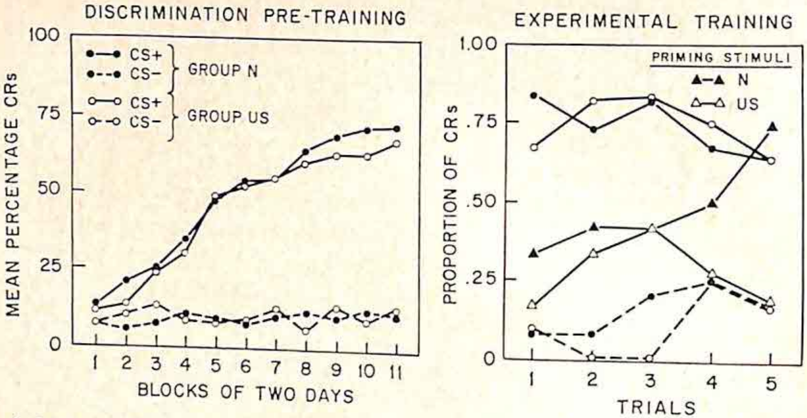

FIGURE 4. Left panel: mean percentage conditioned responses to CS^+ and CS^- plotted over 2-day blocks of discrimination pretraining sessions for the two groups subsequently receiving experimental training in Experiment 4; right panel: proportion of subjects making a conditioned response to the experimental CS on each of the five trials of Day 1, as a function of the priming stimuli occurring within the pairings of the CS and US: no priming stimuli (N), or a priming US. (Also shown is the responding to CS^+ and CS^- on trials occurring proximal to the experimental CS's trials.)

comparably to the CS trained with an unprimed US and to CS^+, while Group US subjects responded comparably to the CS trained with a primed US and to CS^-. Across Trials 4 and 5 for Group N, CS_N was more responded to than was CS^-, $t(11) = 2.69$, $p < .05$, but was equal to CS^+ in mean proportion of responses. In contrast, for Group US, CS_{US} was less responded to than was CS^+, $t(11) = 2.28$, $p < .05$, and was comparable in mean proportion of responses to CS^-, $t(11) = .32$.

One might question whether restricting CRs to those responses occurring within 1,200 msec of onset of CS_{US} may have excluded some responses from measurement that would otherwise have indicated more conditioning to this CS than was described above. On Trial 5, the priming US (as well as the intense US) was omitted to allow scoring the entire 1,800-msec interval normally preceding the intense US. It was found that, during the interval between 1,200 and 1,800 msec following CS onset, of the 12 subjects in each group, only one more responded to CS_{US} and no more to CS_N over the number indicated on Trial 5 in Figure 4.

On Days 2 and 3 of experimental training, beyond what is reported in Figure 4, there was found to be no difference between the two groups in their mean percentages of responses to CS_N and CS_{US} (Group N's mean = 37.7% and Group US's mean = 35.8%), nor in their discriminative responding to CS^+ and CS^-. The transience of the differential responding to CS_N and CS_{US} should caution interpretation. But it was anticipated that various factors in the conditions of training for Group US would work against a priming effect. These include the possibility that the priming US would itself promote some conditioning, particularly at the shorter CS–US interval than that of the more intense US, and the possibility that with a consistent sequence of two USs the first would eventually come to act as a second-order reinforcer signaling the more intense US to follow. The elaborate pretraining procedure was in fact largely calculated to reduce the likelihood of the latter effect. It can only be acknowledged that the priming manipulation did not persistently override these, or other, possible influences.

A possible interpretation of Experiments 1, 2, and 3 that has not been mentioned is that a US–CS–US sequence of stimuli produces inhibitory conditioning as a result of the initial backward arrangement of US followed by CS. The priming-like decrement of the present experiment is of consequence in that it was found in the absence of the latter arrangement. However, as this experiment was the least definitive in outcome, it need be added that the possibility of backward inhibitory conditioning as an explanation for the first three experiments is highly speculative. In order to account for the results of these studies, one has to make certain as-yet-untested assumptions. For example, relevant to Experiment 2, one would have to assume that the distractor stimulus blocked conditioning of inhibition to the CS. Equally as tenuous is the basic assumption that the associative product of backward pairings must necessarily be inhibitory. Although an inhibitory product has been reported (e.g., Siegel & Domjan, 1971) following extensive training in some situations, recent evidence (e.g., Heth, 1976; Wagner & Terry, 1975) suggests that a US administered prior to a CS (such as the priming US) can under some circumstances produce excitatory conditioning to the subsequent CS. Indeed, the theoretical proposition offered by Wagner and Terry (1975) would argue that, if the pretrial US was retained in STM long enough to prime the subsequent US, then there should have been some conjoint "rehearsal" of the pretrial-US and CS representations, leading to excitatory conditioning between the two stimuli. As this excitatory product was not evidenced in the present studies, it must be suggested that the decrement in conditioning due to priming of the second US more than offset any excitatory backward conditioning that may have occurred between the first US and the CS.

General Discussion

The present series of studies demonstrates that the amount of Pavlovian conditioning that accrues with pairings of a CS and US can be attenuated by priming the US with an earlier US presentation. In both within- and between-subjects comparisons, less conditioning was observed when CS–US pairings were preceded by US-alone presentations than when they were not so preceded (Experiments 1 and 2). The decremental influence of a priming US was found to be stimulus specific, as less conditioning was observed if CS–US pairings were preceded by occurrence of the same US than if preceded by occurrence of a different US (Experiment 3). However, the reduction in conditioning produced by a priming US could be removed by the interpolation of a novel stimulus between such US and the subsequent CS–US presentations (Experiment 2). All of these results were observed both on trials in which the experimental manipulations occurred and on test trials in the absence of the differential pretrial treatments. Thus, the priming deficit appeared in the stable reaction to the CS apart from factors that might uniquely influence performance only following the priming US, or only in the absence of a component of training. Finally, diminished conditioning was obtained when a priming US occurred between the presentations of the CS and conditioning US, that is, within the CS–US trial (Experiment 4), showing that priming-like effects are not peculiar to a particular arrangement of the priming US and the CS.

These findings support the hypothesis (Wagner, 1976) that the self-generated priming of a US in STM can reduce the effectiveness of the US in instigating processing steps necessary for conditioning to occur. The results of the several manipulations employed are readily predicted if one assumes that priming acts via a short-term memory mechanism. Thus, the decrement in stimulus processing should be restricted to those conditioning USs that immediately follow the priming US and should not affect other USs more remotely experienced by the subject. The decrement should be specific to a repetition of the stimulus already represented in STM and be removed by clearing the priming representation from STM by subsequent presentations of novel stimuli.

While there are possible alternatives to the priming hypothesis for the results of each experiment, the overall pattern of data cast doubt on the adequacy of any one alternative. The major evidence against each may be summarized here.

As argued in Experiment 1, a reduction in CS–US contingency (Rescorla, 1967) notion would not predict a decrement in conditioning to one CS paired with a primed US but not to a second CS paired with an unprimed US, when found in a within-subjects design. This notion also would not anticipate the distractor's effects in Experiment 2 nor the results of Experiment 4 when both the priming and conditioning USs were signaled by the CS.

It was earlier argued that the observed effects of interpolating a distractor stimulus

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Relative Persistence as a Function of Order of Reinforcement Schedules

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Stimulus analyzer theory as proposed by Sutherland and Mackintosh makes the unique prediction that the first-experienced reinforcement schedule will influence resistance to extinction more than subsequent schedules. Specifically, it predicts that giving continuous reinforcement prior to partial reinforcement (C–P) will result in faster extinction than giving continuous reinforcement after partial reinforcement (P–C). Results presently reported of runway acquisition and extinction indicate the opposite: C–P consistently produced substantially greater persistence than P–C. The theoretical implications of these results are elaborated.

To what extent is behavioral persistence dependent upon the early, as opposed to the more recent, reinforcement history of the organism? Is persistence more influenced by initial reward schedules or by those in effect just prior to extinction, that is, by "primacy" or by "recency"? The stimulus analyzer theory proposed by Sutherland and Mackintosh (1971) makes the unique prediction that primacy variables would be the stronger. Stimulus analyzer theory proposes that two processes occur in any learning situation: The organism learns to attend to certain features of the stimulus environment (to switch in certain analyzers) and to attach certain responses to these features. The strength of the association between responses and features is increased by reward and decreased by nonreward. The strength of a given analyzer depends upon the consistency with which its output is correlated with important environmental consequences, that is, by its ability to predict further events. Under continuous reinforcement (CR) whichever analyzers happen to be dominant initially will be strengthened, and other analyzers will be proportionately weakened.

Consider the implication for a runway situation. Under a CR schedule the running response is presumed to be conditioned to a small set of dominant stimulus features. In the case of partial reinforcement (PR) the dominant analyzers are not consistently rewarded and the response becomes attached to a greater number of nondominant relevant analyzers. Under the assumption that during extinction "animals only notice a limited number of features on any one trial, PR animals will take longer to extinguish, since they must again search through all the different features to which the response has been attached before they can learn to stop running" (Sutherland & Mackintosh, 1971, p. 341). It also follows from stimulus analyzer theory that when PR is preceded by CR (C–P) the analyzer that were conditioned during CR will be effective during a substantial number of early PR trials; thus, nondominant analyzers will be less effectively attached to the response than would be the case if PR had preceded CR (P–C). Thus the theory predicts that "animals given CR trials before PR will extinguish faster than animals given CR after PR" (Sutherland & Mackintosh, 1971, p. 347).

Results that appeared to support this prediction were reported by Sutherland, Mackintosh, and Wolfe (1965). However, the interpretation of these results was somewhat compromised by the fact that the P–C group was running much faster than the C–P group at the end of acquisition. Theios and Mc-

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Ginnis (1967) repeated portions of the Sutherland et al. experiment and found that, when the extinction data were adjusted for differences in terminal acquisition, Group C–P was actually more persistent than Group P–C, a result that was, of course, contrary to that predicted by stimulus analyzer theory.

Sutherland and Mackintosh (1971) defend the theory against the apparently contradictory evidence supplied by Theios and McGinnis on two grounds. They contend that the Sutherland et al. "results cannot be explained by differences in rate of extinction, since running times after 20 extinction trials were the same for Groups P–C and C–P but diverged from there on" (1971, p. 349). They argue further that the Theios and McGinnis experiment may have failed to confirm the Sutherland et al. experiment because Theios and McGinnis used a low criterion of extinction and failed to give enough extinction trials for the expected effect to appear. While the first point seems cogent, the latter is probably debatable. In any event, it is quite apparent that stimulus analyzer theory makes a unique prediction regarding the persistence-producing effects of order of CR and PR phases and that the available experimental evidence has not resolved the issue. The primary purpose of the present experiment was to provide a further test of the effects of order of CR and PR phases on resistance to extinction. In addition, the experimental design incorporated a group that received continuous nonreward in Phase 1 and thus provided a test of the reliability of the initial nonreward effect (INE) reported by Spear, Hill, and O'Sullivan (1965) and Spear and Spitzner (1967).

METHOD

Subjects

The subjects were 96 male albino rats obtained from the Holtzman Co. of Madison, Wisconsin. They were 90–120 days of age at the beginning of pretraining and were housed in individual cages throughout the experiment.

Apparatus

The apparatus was a straight, gray runway 78 in. long, 4.5 in. wide, and 6 in. high (198.12 × 11.33 × 15.24 cm) with Plexiglas covering the entire length. Manually operated guillotine doors separated the first 13.75 in. (34.93 cm) and last 15.75 in. (40 cm) of runway into startbox (SB) and goalbox (GB) compartments, respectively. Response times were recorded on three standard electric timers encased in a soundproof console. Clock 1 (start time) was initiated by raising the SB door. A photocell 6 in. (15.24 cm) beyond the SB door stopped Clock 1 and started Clock 2. Clock 2 (run time) was stopped and Clock 3 started by the animal breaking a photobeam 42 in. (106.68 cm) from the startbox door. The interruption of a photobeam 60 in. (152.4 cm) from the SB door stopped Clock 3 (goal time). The goal cup (.375 in. deep × .75 in. long × 2.5 in. wide—.953 × 1.91 × 6.35 cm) was 3.25 in. (8.26 cm) beyond the last photocell and was visually obscured by a wooden block 1.25 in. high (3.18 cm).

Procedure

The 96 rats were trained in four overlapping replications, with each replication containing 24 animals. Each replication was further divided into four squads with each squad containing one rat from each experimental group.

Pretraining. On Day 1 of the experiment ad lib food was removed, and the animals were placed on a daily maintenance diet of 1-hr. free access to lab chow for the remainder of the experiment. On Days 23–25 the rats were individually handled for 5 min.; on Day 26, in groups of four they received 30 min. of unrestrained exploration in the apparatus. In addition to the maintenance diet, they received 10 45-mg Noyes pellets in the home cage on Days 23–26.

Acquisition. Following pretraining the rats were randomly assigned to one of six experimental groups. Groups P–C received 60 PR trials in Phase 1 followed by 100 CR trials in Phase 2; Group C–P received 100 CR trials followed by 60 PR trials; Groups N–C and N–P received 60 nonrewarded trials followed by 100 CR trials or 100 PR trials, respectively; and Groups C–C and P–P received 160 CR or PR trials, respectively. A 50% quasi-random PR schedule was employed during PR training, with a maximum of 2 successive nonrewarded trials preceding a rewarded trial.

During acquisition training, 10 trials were administered each day for 16 days. On rewarded trials the animals received 10 sec of free access to 45-mg Noyes pellets. On nonrewarded trials they were confined to the goalbox for 10 sec. On the 60 nonrewarded trials for Groups N–C and N–P, they were allowed a maximum of 60 sec in the runway but were removed from the runway following a 10-sec goalbox confinement if they reached the goal area. Daily trials were administered in a lock-step manner such that every rat in a squad received Trial 1 before any of them received Trial 2, and so on, creating an intertrial interval of approximately 6–7 min. The daily order of running the subjects within a squad was randomly determined.

On a typical training day each squad was transported to the experimental chamber, and the first daily trial was initiated following a 10-min. waiting period. A trial consisted of removing the rat from the home cage and placing it in the SB facing the SB door. Following a 2-sec delay, the SB door was raised allowing access to the runway. The SB and GB doors were lowered when the subject left the SB and interrupted the final photobeam, respectively. After experiencing the GB conditions scheduled for that trial, the subject was removed from the runway and returned to the home cage to await the next trial.

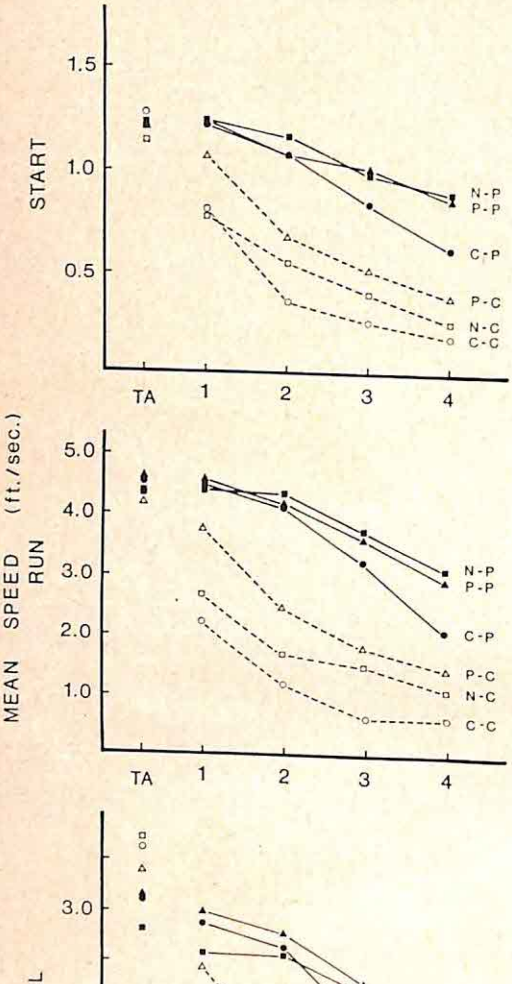

FIGURE 1. Mean speed in start, run, and goal sections during last 10 trials of acquisition and 40 extinction trials as a function of training treatment. (N = nonrewarded trials, P = partial reinforcement, C = continuous reinforcmeent.)

Following acquisition, subjects received 4 days of extinction training at 10 trials per day. The extinction procedure was identical to that employed in acquisition with the exception that food was never available.

RESULTS

For the purpose of statistical analyses, start, run, and goal times were transformed into ft/sec scores by dividing each time into the value of its corresponding runway-section length.

Acquisition. Figure 1 depicts the terminal acquisition speeds and extinction performance for the six experimental groups. In order to assess the presence of differential terminal acquisition speeds between groups, the six groups were compared on start, run, and goal measures utilizing the mean of the last 10 acquisition trials as the index of terminal performance. Only the goal measure yielded a statistical difference between groups on acquisition speeds, $F(5, 90) = 5.99$, $p < .01$. Subsequent Newman–Keuls comparisons between groups indicated that (a) the performance of Group N–P was significantly below that of Groups P–C, C–C, and N–C, and (b) Groups C–P and P–P ran significantly slower than Groups C–C and N–C, $p < .05$ in each case. All other comparisons between groups did not reach statistical significance, (e.g., there were no terminal acquisition differences between C–P and P–C treatments in any of the runway segments).

Extinction. In order to assess the extinction performance differences evident in Figure 1, a 6 (Groups) × 4 (Trial Blocks) analysis of variance was performed on the start, run, and goal measures. Significant main effects were obtained for groups (start, $F(5, 90) = 42.16$, $p < .01$; run, $F(5, 90) = 54.48$, $p < .01$; goal, $F(5, 90) = 39.99$, $p < .01$), trials (start, $F(3, 270) = 174.85$, $p < .01$; run, $F(3, 270) = 191.55$, $p < .01$; goal, $F(3, 270) = 191.95$, $p < .01$), and Group × Trial Blocks interaction (start, $F(15,$

270) = 3.61, $p < .01$; run, $F(15, 270) = 5.28$, $p < .01$; goal, $F(15, 270) = 6.08$, $p < .01$). The simple effects for groups at each level of the trials factor were calculated on each dependent measure. F tests indicated significant ($p < .01$) simple effects for the groups variable during each extinction block in each runway segment. The pattern of significant ($p < .05$) Newman–Keuls comparisons for adjacent groups revealed that (a) N–P and P–P were not reliably different; (b) P–P animals ran reliably faster than C–P animals during the final extinction block (start and goal speeds); (c) C–P was reliably more persistent than P–C throughout all extinction blocks on the goal-speed measure and during Extinction Blocks 2 and 3 on the start and run measures; (d) P–C was reliably faster than N–C during the first three extinction blocks (goal speed); (e) N–C was more persistent than C–C during Extinction Block 2 (all measures) and Block 3 (run speed).

DISCUSSION

Persistence of C–P versus P–C. The conclusion regarding this critical comparison is unambiguous: Giving CR prior to PR results in substantially greater persistence than giving PR prior to CR. These results are contrary to the explicit prediction made by stimulus analyzer theory and support the validity of the Theios and McGinnis results with rats in a straight alley and Cotler and Nygaard's (1969) results with human subjects in an instrumental conditioning task. However, the size of the effect in the present experiment is much larger than that reported by Theios and McGinnis or by Theios (1962), where substantial effects of the interpolation of CR did not appear until after 35 extinction trials.

Sutherland and Mackintosh's criticisms of the Theios and McGinnis experiment and their defense of the Sutherland et al. results do not seem to be valid in the context of the present results. Consider, first, that our C–P versus P–C differences are the opposite of those obtained by Sutherland et al., and that they appear from the very first extinction block. Furthermore, it should be emphasized that our results are not complicated by terminal acquisition differences between C–P and P–C treatments. Stimulus analyzer theory however predicts that "a difference between two groups should be manifested later in extinction when the dominant analyzers are switched off" (Sutherland & Mackintosh, 1971, p. 349). In this regard, although like Thelios and McGinnis we gave "only" 40 extinction trials, it does not seem reasonable to attribute our results to failure to provide an opportunity for the effect anticipated by analyzer theory to show itself. The contrary differences that were obtained were large and persistent. More importantly, there is internal evidence that 40 trials provided the extinction experience required by analyzer theory, since effects that would be expected to appear late in extinction were indeed present during the last extinction block. Specifically, C–P showed significant response decrement compared with P–P, an effect that stimulus analyzer theory contends would occur only late in extinction.

Experimental group versus C–C control. The immediately obvious finding is that all groups that had experience with nonreward prior to extinction were more persistent than Group C–C; that is, the usual partial reinforcement extinction effect (PREE) was obtained for all groups. Some comments on the relative size of the PREE in the various groups are in order. The increased persistence of Group N–C represents a further confirmation of the INE of Spear and his colleagues. The occurrence of the INE presents theoretical difficulties for Amsel's frustration theory, which requires that anticipatory reward (r_g) be conditioned to approach responses prior to the experiences of nonreward in order for persistence-producing frustration (r_f) to be counterconditioned to the approach response (Amsel, 1967).

Spear's explanation of the INE rests upon the construct of a central representation of reinforcer magnitude, r_g, (including zero magnitude), which "is not eliminated completely from S's response repetoire as a consequence of a subsequent, different r_g" (1967, p. 231). The INE is also easily accommodated by Capaldi's stimulus aftereffect theory, which specifies that a critical mechanism in extinction is differential generalization decrement between stimulus-specific memories acquired during training and the stimuli that occur during extinction (Capaldi, 1966, 1967, 1971). Capaldi (1971) has shown that the INE can be demonstrated with four training trials and a single NR transition (NNRR). The fact that interspersing of N and R trials (P–C) is even more persistence-producing than a greater number of continuous N trials (N–P) is explained by Capaldi as due to the differential number of NR transitions in the two treatments.

The presence of a significant PREE for group P–C is consistent with previous runway studies with rats (Ross, 1964; Sutherland et al., 1965; Theios, 1962; Theios & McGinnis, 1967; Black & Spivey, Note 1), free operant studies with rats (Keller, 1940) and pigeons (Jenkins, 1962), classical eyelid conditioning in humans (Perry & Moore, 1965) and human instrumental conditioning (Cotler & Nygaard, 1969). This robust effect requires that any theory of partial reinforcement include mechanisms whereby the effects of nonreward are stored in long-term memory and are available for retrieval by reinstatement of similar stimuli during extinction. Several extant theories incorporate such mechanisms (e.g., frustration theory, Amsel, 1967; stimulus aftereffects theory, Capaldi, 1967, 1971; Spear's, 1967, retention of reinforcer magnitude mechanism; and stimulus analyzer theory, Sutherland & Mackintosh, 1971); thus the occurrence of a significant PREE for Group P–C does not permit us to discriminate among these theories. Similarly, all theories of partial reinforcement would expect Group C–P to show a PREE, and the empirical results support this expectation for rats in a straight alley (Sutherland et al., 1965; Theios & McGinnis, 1967; Black & Spivey, Note 1), for operant conditioning of rats and pigeons (Hothersall, 1966; Jenkins, 1962), and for classical and instrumental conditioning of humans (Cotler & Nygaard, 1969; Perry & Moore, 1965).[1]

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[1] Strictly speaking, since Hothersall did not include a CR control group, his experimental design does not permit statements about the PREE per se.

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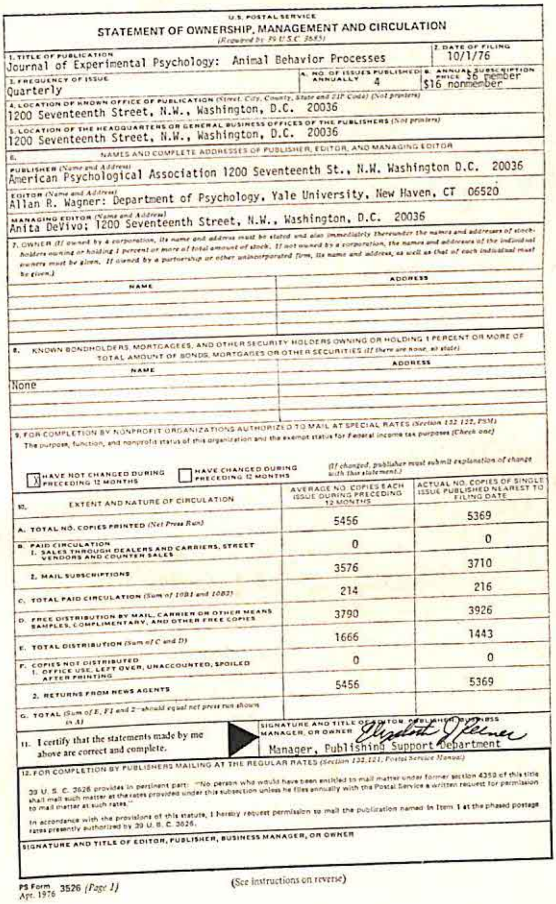

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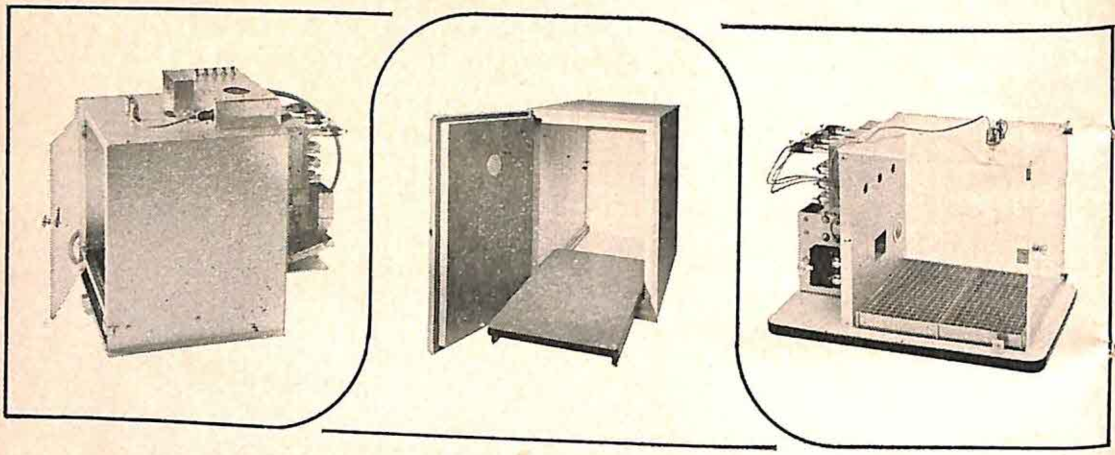

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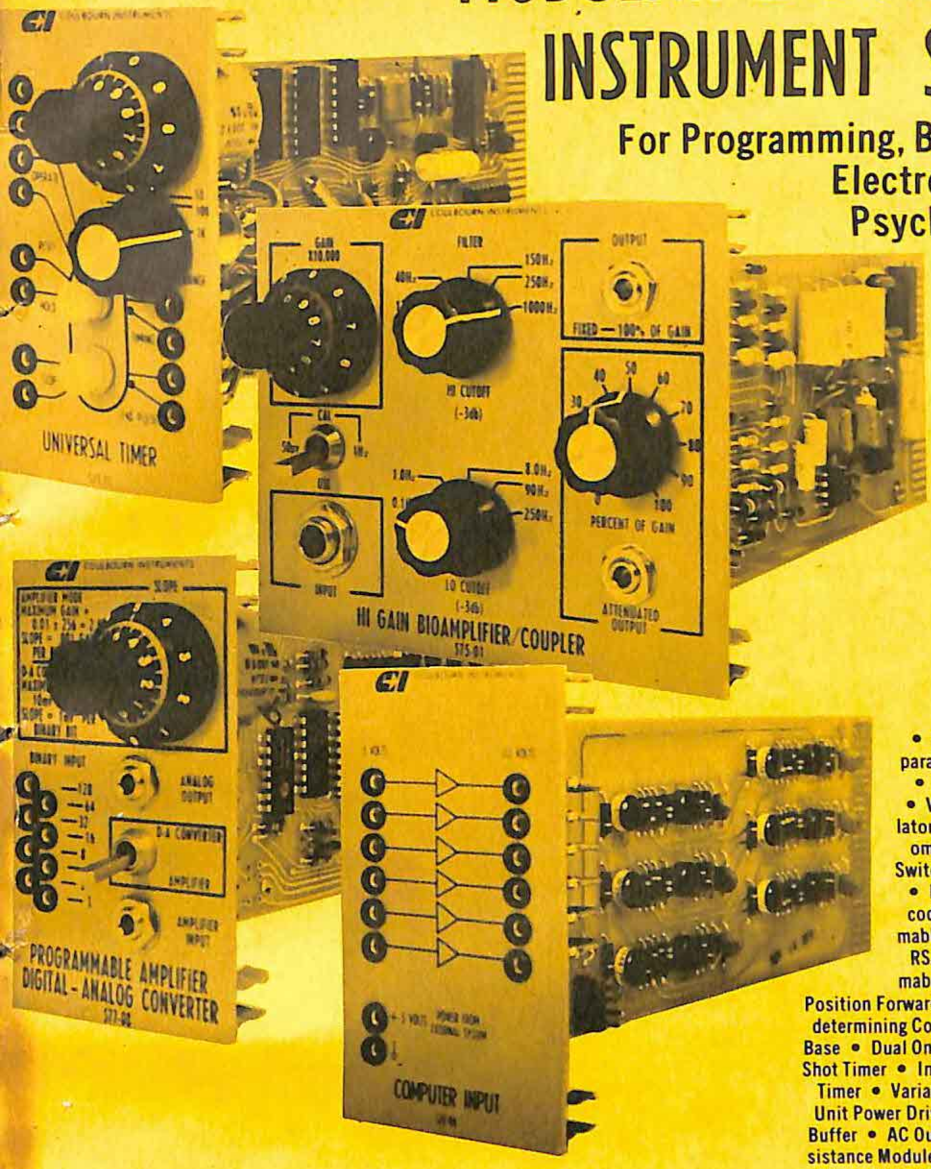

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